STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

EDWARD W. SCRIPTURE, PH. D.

Director of the Laboratory of the Department of Philosophy

1897

VOL. V.

YALE UNIVERSITY

NEW HAVEN, CONN.

THE NEW ERA PRINTING COMPANY, LANCASTER, PA.

CONTENTS.

Researches on acoustic space, by M. MATSUMOTO, ... 1
On binaural space, by E. W. SCRIPTURE, ... 76
The size-weight illusion among the blind, by JAMES F. RICE, M.A.,
Cerebral light, by E. W. SCRIPTURE, ... 81
Researches on memory for arm movements, by E. W. SCRIPTURE, 88
W. C. COOKE and C. M. WARREN, ... 90
Principles of laboratory economy, by E. W. SCRIPTURE, . . . 93
Notes, ... 104

RESEARCHES ON ACOUSTIC SPACE.[1]

BY

MATATARO MATSUMOTO.

E. WEBER[2] seems to have been the first to call attention to the errors in localizing sounds. The particular problems involved seem to be two: 1. the perception of the direction from which a sound comes; and 2. the perception of its distance. The investigations described in the following pages were made in the attempt to contribute data toward the solution of these two problems. The work was begun (1894) in the Psychological Laboratory of the Imperial University of Japan (Tokyo) at the suggestion of Professor MOTORA; the greater part of the work, however, was done during the years 1896–1898 in the Psychological Laboratory of Yale University under the supervision of its director, E. W. SCRIPTURE. Many suggestions were also received from Professor LADD.

I. PRELIMINARY INVESTIGATIONS.

The first series of experiments was conducted according to PREYER'S statistical method.[3] Instead of PREYER'S sound-helmet, a hollow spherical cage was devised as is shown in Figure 1. The imaginary surface of the sphere whose diameter is 1.35^{m} is divided into 8 equal parts by 4 vertical great circles. The surface is again divided horizontally by the equator and by two small circles parallel to the equator at a distance of 45° from poles. The intersecting points of these vertical and horizontal circles correspond to the 26 terminal points of 13 axes or diameters of the sphere. These 13 axes may be divided into three classes.

I. Three primary axes which cut each other at right angles.

(*a*) The frontal axis, or the diameter of the sphere from right to left in the plane of the equator. As this line corresponds to an imaginary line

[1] Submitted to the Tokyo Imperial University as a thesis for the degree of Hakush (Ph.D.).

[2] ED. WEBER, *Ueber den Mechanismus des Gehörorgans*, Ber. d. kgl.-sächs. Ges. der Wiss., math.-phys. Classe, 1851, 29.

[3] PREYER, *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Archiv f. d. ges. Physiol. (Pflüger), 1887 XL 586.

drawn through the external openings of the two ears of the subject seated in the cage, it may be called the auditory axis or the *rl* (right-left) axis.

(*b*) A vertical diameter which intersects the frontal axis at its middle point. This may be called the vertical or the *ou* (over-under) axis.

The plane determined by the two axes *rl* and *ou* is called the frontal plane.

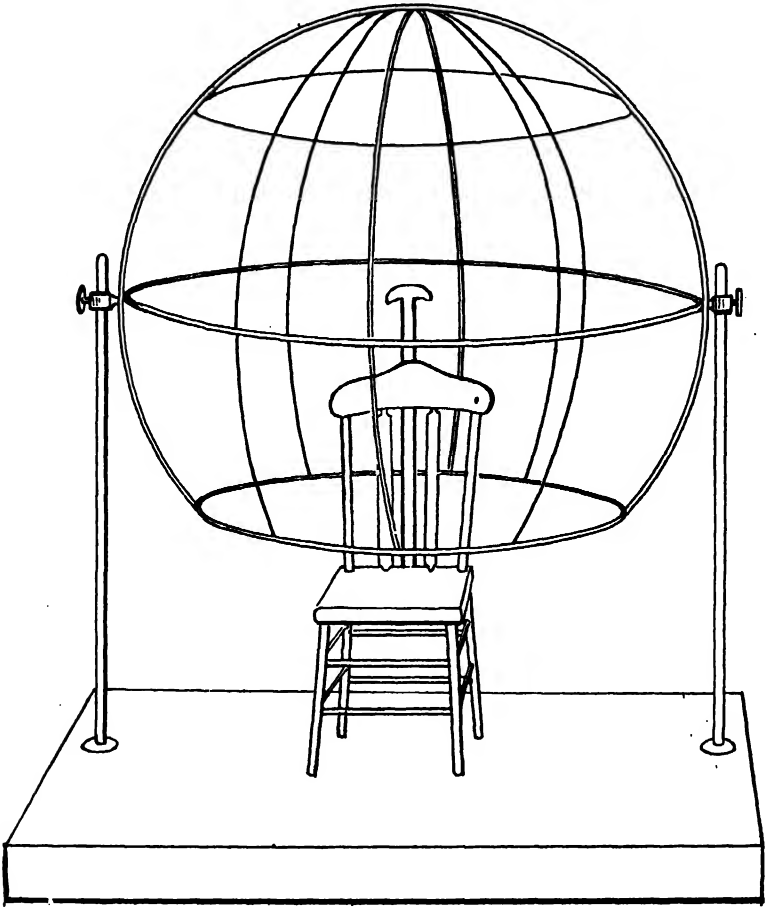


Fig. 1.

(*c*) A horizontal diameter drawn perpendicular to the frontal plane, through the intersecting point of the frontal and the vertical axes. This may be called the sagittal or the *fb* (front-back) axis.

The plane determined by the sagittal and the vertical axes is called the the sagittal or median plane, while the plane determined by the sagittal

and the frontal axes is called the horizontal plane. In the present essay no use will be made of median, frontal or horizontal planes except the primary ones, as just defined; the terms are always to be understood in this way.

The above three axes correspond to the *X*, *Y*, *Z* axes of the Cartesian system of coördinates, and represent the fundamental axes upon which our standard space is constructed with ourselves as the center.

II. Six secondary axes, every two of which lie in the plane determined by the primary axes and cut each other at right angles.

(*d*) Two secondary frontal axes. These are the diameters lying in the frontal plane at the distance of 45° from the frontal and from the vertical axis.

(*e*) Two secondary sagittal axes. These are the diameters lying in the median plane at the distance of 45° from the sagittal and from the vertical axis.

(*f*) Two secondary horizontal axes. These are the diameters lying in the horizontal plane at the distance of 45° from the sagittal and from the frontal axis.

III. Four tertiary axes. These are the diameters lying at 45° from the three neighboring secondary axes in each case.

These thirteen axes are illustrated in the model, Figure 2.

The 26 terminal points of the 13 axes are named in the following way:

I. The 6 terminal points of the 3 primary axes are *f* (front), *b* (back), (over), *u* (under), *r* (right), *l* (left).

II. The 12 terminal points of the 6 secondary axes are:

(*a*) *fo* (front-over), *bu* (back-under), *fu* (front-under), *bo* (back-over).

(*b*) *or* (over-right), *ul* (under-left), *ur* (under-right), *ol* (over-left).

(*c*) *fr* (front-right), *bl* (back-left), *fl* (front-left), *br* (back-right).

III. The 8 terminal points of the 4 tertiary axes are:

(*a*) *for* (front-over-right), *bul* (back-under-left).

(*b*) *fol* (front-over-left), *bur* (back-under-right).

(*c*) *bor* (back-over-right), *ful* (front-under-left).

(*d*) *bol* (back-over-left), *fur* (front-under-right).

The person to be experimented upon is seated in the inside of the cage; his head is adjusted by means of a head-rest fixed to the back of the chair in such a way that his visual axis in the normal position of the body will lie in the median plane, and his auditory axis (an imaginary line drawn through the openings of the ears) with the frontal axis. Then the experimenter gives a short sound at one of the 26 terminal points, and the observer, with his eyes closed, is to judge the direction of the sound. In

my experiments the sound was produced by means of a telephone or a small metallic hammer. Fifty experiments were made for each of the 26 points. The observer was Mr. T. Oku, a student of philosophy.

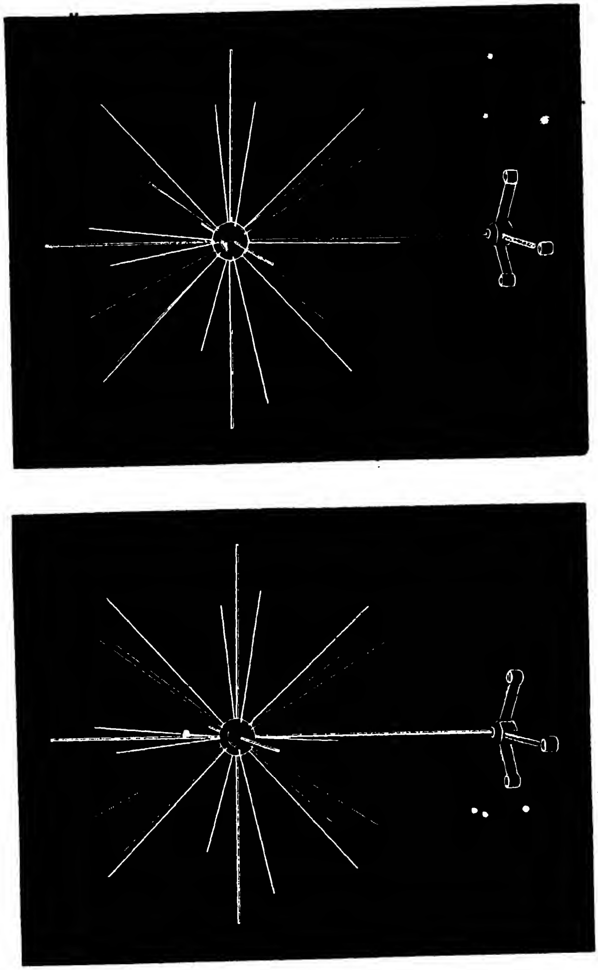


FIG. 2.

(When viewed with a stereoscope the model appears in relief.)

If we could not perceive the direction of sound at all, it would be, as PREYER[1] noticed, theoretically possible that each of the 26 directions would be confused with each of the remaining 25 directions so that there would occur in all $26^2 - 26 = 650$ confusions. Therefore in 676 experi-

[1] PREYER, *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Archiv f. d. ges. Physiol. (Pflüger), 1887 XL 586.

ments the correct judgments would amount only to 26. Or in 1,000 experiments the correct judgments would not exceed 40. This was not the case, for in 1,300 experiments (780 telephone sounds and 520 metallic clicks) it was found that the correct judgments amounted to 768, namely, $\frac{3}{5}$ of the total number instead of $\frac{1}{25}$ as theoretically required. Therefore, the perception of the direction of sound cannot be regarded as purely accidental.

It was noticed that in these experiments none of the 26 directions was actually confused with more than 8 directions. Of 650 possible kinds of errors only 113 kinds were actually observed in our 1,300 experiments and, indeed, many of these 113 kinds occurred only once or twice. What are the remaining kinds of errors which did not occur actually, though they were theoretically possible? This question leads to a very important principle in the perception of the direction of sound. In the experiments the following results were noticed.

1. No sound on the right side was perceived as being on the left side, and no sound on the left side was perceived as being on the right side. That is, none of the series, *r*, *fr*, *for*, *or*, *bor*, *br*, *bur*, *ur*, *fur*, was confused with any of *l*, *fl*, *fol*, *ol*, *bol*, *bl*, *bul*, *ul*, *ful*, and vice versa. As there are 9 *r*-directions and 9 *l*-directions, then 162 (i. e., 2 × 9 × 9) kinds of errors must be subtracted from the total number of errors theoretically possible.

2. No sound on the right or the left side was localized in the median plane. That is, none of the above series was confused with the series in the median plane *f*, *fo*, *o*, *bo*, *b*, *bu*, *u*, *fu*. Therefore we must subtract 144 (i. e., 2 × 9 × 8) kinds of errors from the total number theoretically possible.

3. No sound in the median plane was localized on the right or left side of the plane. That is, none of the series *f*, *fo*, *o*, *bo*, *b*, *bu*, *u*, *fu*, was confused with *l*, *fl*, *fol*, *ol*, *bol*, *bl*, *bul*, *ul*, *ful*, *r*, *fr*, *for*, *or*, *bor*, *br*, *bur*, *ur*, *fur*. Here again 144 (i. e., 2 × 8 × 9) kinds must be subtracted from the total number.

Subtracting these 450 kinds of errors from 650 theoretically possible kinds of errors we get 200 kinds of errors as actually occurring. These 200 kinds of errors are those which will be actually observed. They consist of 72 (i. e., 9 × 9 — 9) confusions on the right side, 72 (i. e., 9 × 9 — 9) confusions on the left side and 56 (i. e., 8 × 8 — 8) confusions in the median plane.

In respect to these three fundamental facts the results of my own experiments perfectly agree with those of PREYER.

These facts lead us to believe that the possession of two ears gives us

an important means of perceiving the direction of a sound. When a sounding body is situated in the median plane, there is no difference between the intensities (and the other possible properties) with which vibratory movements arrive at the two ears. But when a sound is situated outside of the median plane the results will be different and the greater the angular distance from the median plane the greater will be the difference. The relative amount of this difference—the binaural parallax—may give us effective data by which we can judge the direction of the sound. If such a supposition be true, the direction of a sound will be best perceived when it is situated in or around the frontal or auditory axis, for here the difference will be greatest; we can also expect that the direction of a sound will be fairly well recognized when it is situated in the sagittal axis, for that axis is unique in its relation to the two ears. Moreover, it may be expected that the direction of a sound in the horizontal plane will be best perceived, for the shape of the pinna is most favorable for receiving a sound in the horizontal plane, especially in the case of binaural perception. Now let us examine the results of our experiments more closely to see whether they support these suppositions.

For each primary axis the ratio of the correct judgments to the total number was as follows: for *rl* axis, $\frac{87}{100}$; for *fb* axis, $\frac{72}{100}$; for *ou* axis, $\frac{63}{100}$. These results show that the sounds in the *rl* axis are best localized, while the sounds in *fb* axis are better localized than those in *ou* axis. In PREYER's experiments the ratio of the correct judgments for *ou* axis was greater than that for *fb* axis.

Again the ratio of the correct judgments for the 8 directions in each primary plane was as follows: for the horizontal plane (*f, fr, r, br, b, bl, l, fl*), $\frac{291}{400}$; for the frontal plane (*o, or, r, ur, u, ul, l, ol*), $\frac{271}{400}$; for the median plane (*f, fo, o, bo, b, bu, u, fu*), $\frac{233}{400}$. The results show that the sounds in the horizontal plane are localized best of all, while the sounds in the frontal plane are better localized than those in the median plane, these results agree with those of PREYER and ARNHEIM.[1]

The influence of the sounds from the right and left sides is so strong that even the ratio of all correct judgments for those 18 directions in which *r* and *l* take part has a greater value than the ratio of all correct judgments for either of the 18 directions in which *f* and *b* or *o* and *u* take part. In the last two groups *r* and *l* do not occur so often as in the first. The ratios of the correct judgments were as follows: for *rl*

[1] ARNHEIM, *Beiträge zur Theorie von Schallempfindungen mittelst der Bogengänge*, Diss., Jena 1887.

18 directions, $\frac{530}{900}$; for *fb* 18 directions, $\frac{497}{900}$; for *ou* 18 directions, $\frac{477}{900}$. Our supposition that the possession of two ears gives us by binaural parallax an important means for perceiving the direction of sound seems to be supported by these results. But this general principle is made more complex by various circumstances. For upon examining the number of the correct judgments in the three sets of hemispheres it was found that:

1. The direction of a sound in the right hemisphere was more correctly judged than that of a sound in the left hemisphere. The correct judgments for *r*, *fr*, *for*, *or*, *bor*, *br*, *bur*, *ur*, *fur* amounted to 278, while the correct judgments for *l*, *fl*, *fol*, *ol*, *bol*, *bl*, *bul*, *ul*, *ful* amounted to 257. If the binaural parallax is an important means of localizing a sound, then it is highly probable that the localization will be more or less influenced by the difference in sensitiveness which exists between the two ears. Although the subject was not examined in this respect, it is probable that there was such a difference.

2. The direction of a sound in the front hemisphere was more correctly judged than that of a sound in the rear hemisphere. The correct judgments for *f*, *fo*, *fu*, *fl*, *fol*, *ful*, *fr*, *for*, *fur* amounted to 260, while those for *bo*, *b*, *bu*, *bol*, *bul*, *bl*, *bor*, *br*, *bur* amounted to 237. This difference probably finds its explanation in the function of the pinnæ whose shape is not favorable for receiving sounds in the rear hemisphere.

3. Lastly, a sound in the lower hemisphere was better localized than a sound in the upper hemisphere. The correct judgments for *fu*, *u*, *bu*, *bul*, *ul*, *ful*, *bur*, *ur*, *fur* amounted to 271, while those for *fo*, *o*, *bo*, *bol*, *ol*, *fol*, *bor*, *or*, *for* amounted to 206. We cannot say that this will always be the case, for in PREYER'S experiments the sounds were better localized in the upper hemisphere than in the lower hemisphere. The results of my experiments might have been more or less influenced by the probable reflection of sound from the lower parts of the apparatus and the floor, in consequence of which the sound might have been peculiarly colored, as it were, according to its position, and better discriminated by the observer. Apart from such influences the results might have been influenced by the form of the pinnæ.

The examination of these three groups of results enables us to say with great probability that the differences in the degrees of sensitiveness in the two ears and the action of the pinnæ are the factors which render our perception of the direction of sound more or less complex.

Table I. summarizes the results of these preliminary experiments. In this table the objective positions of sounds are indicated in the vertical column at the left, and the perceived directions are given in the hori-

TABLE I.

Position of sound.	f	fo	o	bo	b	bu	u	fu	fl	fol	ol	bol	bl	bul	ul	ful	l	fr	for	or	bor	br	bur	ur	fur	r	No. of experiments.
	Direction in which the sound is perceived to lie.																										
f	**40**	3						7																			50
fo	12	**27**	6	3	1			1																			50
o		11	**32**	7																							50
bo			16	**16**	14	4																					50
b	1			4	**32**	12	1																				50
bu					4	**29**	17																				50
u			1		7	10	**30**	2																			50
fu	6	3	2	1	10	1		**27**																			50
fl									**35**	5	1				2	4	3										50
fol									9	**25**	13					2	1										50
ol										4	**30**	4			1		11										50
bol									3	3	18	**12**	7	1			6										50
bl											2	6	**26**	8	3		5										50
bul													1	**29**	19		1										50
ul									1		1		2	10	**34**		2										50
ful									9		5	2	2		5	**24**	3										50
l									1	1	3				3		**42**										50
fr																		**31**	5	1		4			3	6	50
for																		8	**20**	16	2					4	50
or																			1	**31**	2			2		14	50
bor																			3	13	**13**	8	2	2	3	6	50
br																				1		**40**	6	1	1	1	50
bur																					1	1	**40**	8			50
ur																				1		4	12	**27**	1	5	50
fur																		1	5		4	2	2	4	**31**	1	50
r																				2		1		2		**45**	50
Total	59	44	57	31	68	56	48	37	58	38	73	24	38	48	67	30	74	40	34	65	22	60	62	46	39	82	**1300**

zontal columns. The number of judgments of each kind is given by the figures which are found below each perceived direction.

In the same manner as the actual possibility of the confusion of the directions of sounds is limited to certain distinct regions, so the directions which are *most liable* to be confused with each other are also restricted to narrower limits than the regions of actual possibility.

In the following lists the frequency of the confusion between pairs of the 26 directions is shown by the figures. Here it is regarded as a matter of indifference whether—for example—*fr* is confused with *for*, or *for* with *fr*, and likewise for any other pairs of directions that may be confused.

LIST I.

Confusions in the Median Plane.

Confusion between *f* and *fo* (15), *fu* (13), *b* (1).
" " *fo* and *o* (17), *fu* (4), *bo* (3); *b* (1)
" " *o* and *bo* (23), *fu* (2), *u* (1).
" " *bo* and *b* (18), *bu* (4), *fu* (1).
" " *b* and *bu* (16), *fu* (10), *u* (8).
" " *bu* and *u* (27), *fu* (1).
" " *u* and *fu* (2).

LIST II.

Confusions in the Left Hemisphere.

Confusion between *l* and *ol*(14), *bol* (6), *ul*(5), *bl*(5), *fl*(4), *ful*(3), *fol*(2), *bul*(1).
" " *fl* and *fol*(14), *ful*(13), *bol*(3), *ul*(3), *ol*(1).
" " *fol* and *ol*(17), *bol*(3), *ful* (2).
" " *ol* and *bol*(22), *ful*(5), *ul* (2), *bl*(2).
" " *bol* and *bl*(13), *ful*(2), *bul*(1).
" " *bl* and *bul*(9), *ul* (5), *ful*(2).
" " *bul* and *ul*(29).
" " *ul* and *ful*(5).

LIST 3.

Confusions in the Right Hemisphere.

Confusion between *r* and *or*(16), *ur* (7), *bor*(6), *fr*(6), *for*(4), *br* (2), *fur*(1).
" " *fr* and *for*(13), *fur* (4), *br*(4), *or*(1).
" " *for* and *or*(17), *bor* (5), *fur*(5).
" " *or* and *bor*(15), *ur* (3), *br* (1).
" " *bor* and *br*(8), *fur* (7), *bur*(3), *ur*(2).
" " *br* and *bur*(7), *ur* (5), *fur*(3).
" " *bur* and *ur*(20), *fur* (2).
" " *ur* and *fur*(5).

These results show that the points which are most liable to be confused with each other are those which are situated nearest to each other in the region of actual possibility of confusion. This is in accordance with our view

that the localization depends upon the binaural parallax, for the sounds at the points situated nearest to each other in the regions of actual possibility of confusion are those which are more nearly equal in their relations to the two ears than the sounds at other points. Moreover, we can notice a certain similarity in the three lists above. The errors that occur most frequently in the second list (first two columns) are of similar kinds with the most frequent errors (first two columns) in the third list. Moreover, if we disregard the question of right and left—thereby cutting off *l* and *r* from the members in the second and third lists, then the most frequent errors (first column) will be the same in all three lists. In other words, the errors which occur most frequently in the median plane are repeated with almost the same regularity in the left and right hemispheres. The results are arranged in Table II.

In connection with the above three lists it is interesting to know the relation of the angular magnitude of the error to its frequency. In the median plane the *possible* frequencies of the errors out of a total of 64 are as follows: for 180°, 8 times, or $12\frac{1}{2}$%; for 135°, 16 times, or 25%; for 90°, 16 times, or 25%; for 45°, 16 times, or 25%; for 0°, 8 times, or $12\frac{1}{2}$%.

In the actual experiments, however, 400 judgments were distributed as follows: for 180°, 3 times, or $\frac{7}{10}$%; for 135°, 13 times, or 3%; for 90°, 20 times, or 5%; for 45°, 131 times, or 33%; for 0°, 233 times, or 58%.

Comparing these two series it becomes evident that in reality the errors are not evenly distributed. The error of 0° magnitude (i. e., correct judgment) is by far the most frequent; the error of 45° (next to the smallest magnitude) comes next to it, and the errors of greater magnitudes occur less frequently as the magnitude increases.

The same observation is to be made concerning the errors in separate hemispheres. If the errors were evenly distributed the frequencies for the right and left hemispheres would be as follows: for 119°, 2 × 4 times, or 5% of the total number; for 98°, 2 × 8 times, or 10%; for 90°, 2 × 12 times, or 15%; for 85°, 2 × 8 times, or 10%; for 60°, 2 × 8 times, or 10%; for 59°, 2 × 8 times, or 10%; for 45°, 2 × 24 times, or 30%; for 0°, 2 × 9 times, or 11%.

In the actual experiments, however, 900 judgments were distributed as follows: for 119°, 9 times, or 1% of the total number; for 98°, 7 times, or $\frac{8}{10}$%; for 90°, 30 times, or $3\frac{3}{10}$%; for 85°, 8 times, or $\frac{9}{10}$%; for 60°, 18 times, or 2%; for 59°, 23 times, or $2\frac{5}{10}$%; for 45°, 270 times, or 30%; for 0°, 535 times, or 59%.

Just as in the median plane, the errors of smaller magnitndes happen

TABLE II.

Position of sound.	*f*	*fo*	*o*	*bo*	*b*	*bu*	*u*	*fu*	*l*	*fl*	*fol*	*ol*	*bol*	*bl*	*bul*	*ul*	*ful*	*r*	*fr*	*for*	*or*	*bor*	*br*	*bur*	*ur*	*fur*
f	**40**																									
fo	15	**27**																								
o		17	**32**																							
bo		3	23	**16**																						
b	1	1		18	**32**																					
bu				4	16	**29**																				
u			1		8	27	**30**																			
fu	13	4	2	1	10	1	2	**27**																		
l									**42**																	
fl									4	**35**																
fol									2	14	**25**															
ol									14	1	17	**30**														
bol									6	3	3	22	**12**													
bl									5			2	13	**26**												
bul									1				1	9	**29**											
ul									5	3		2		5	29	**34**										
ful									3	13	2	5	2	2		5	**24**									
r																		**45**								
fr																		6	**31**							
for																		4	13	**20**						
or																		16	1	17	**31**					
bor																		6		5	15	**13**				
br																		2	4		1	8	**40**			
bur																						3	7	**40**		
ur																		7			3	2	5	20	**27**	
fur																		1	4	5		7	3	2	5	**31**

also in the both hemispheres more frequently than the errors of greater magnitudes. The exceptionally great percentage for 90 degrees arises from the familiar confusions between front and back and between above and below ; these will be considered in detail later.

From these results it follows that the smaller the angular distance between the two points, the greater is their confusion with each other. Though this fact is a matter of common experience, the experimental determination of it is very important.

The frequencies of the errors relating to the magnitude, which we actually observed in our experiments, are shown in Table III.

TABLE III.

	0°	45°	59	60°	85	90°	98	119°	135°	180°	Number of experiments
l	42	7	1								50
r	45	5									50
bl	26	19		5							50
br	40	7		2	1						50
ol	30	19				1					50
or	31	17				2					50
fl	35	12		3							50
fr	31	14		1		4					50
ul	34	12		3		1					50
ur	27	18		4		1					50
bol	12	25	6		3	4					50
bor	13	21	6			5	2	3			50
fol	25	22	1			2					50
for	20	24	4			2					50
bul	29	20	1								50
bur	40	9				1					50
ful	24	14	3		2		5	2			50
fur	31	5	1		2	7		4			50
bo	16	30				4					50
o	32	18									50
fo	27	18				4			1		50
f	40	10									50
u	30	12				7				1	50
b	32	16				1				1	50
bu	29	21									50
fu	27	6				4			12	1	50
Total	768	401	23	18	8	50	7	9	13	3	1300

The foregoing preliminary experiments have shown that the difference between the sensations with which a sound is heard in the two ears must be regarded as the fundamental datum for localizing the sound.

The next step must therefore be a closer examination of this datum of

localization. There are four characteristics of sound-waves by which one sound may be discriminated from another, namely, intensity, pitch, phase and complexity (or timber). The localization of a sound must be based upon a difference in one or more of these four characteristics.

Of these four characteristics it was the question of intensity to which my chief attention was paid in the further experimental work, for from the nature of the subject the problem could be more definitely studied in reference to this characteristic than to the other ones.

II. Dependence of the localization of a perceived sound upon the relative intensities of the sounds heard by the two ears.

We have seen in our preliminary experiments that a sound in the median plane is never localized on the right or the left side and a sound on the right or the left side is never localized in the median plane, and we have assumed that these facts depend upon the peculiar relation between the intensities with which the ears are excited by a sound in the median plane. Now the queston arises whether we do always localize the perceived sound in the median plane when both ears are excited with the same intensity. The following experiments were conducted to get an answer to this question.

1. *Dependence of the localization of a sound in the median plane upon the equal intensities of the impressions in the two ears.*

Each of the primary circles of the spherical cage was divided into degrees. In the horizontal circle the front (*f*) was taken as 0° and the degrees were counted on both sides of the circle from front to back, the back being 180°. In the frontal circle the top was taken as 0° and the degrees were counted from the top downward, the point opposite to the top being 180° and that horizontally either right or left 90°. In the median circle the top was taken as 0°, and the degrees were counted from the top downward, either front or back being 90° and the point opposite to the top 180°.

Two telephones were placed at two symmetrical points of the same circle. The head of the observer was adjusted as in the preliminary experiments. The two telephones were sounded with equal intensities for two seconds. The observer was to judge the direction of the sound. The points at which the telephones were placed are given in Table IV.

For each pair of positions 4 to 8 experiments were made; the total number of experiments was 125. To eliminate the effect of suggestion

and practice, the experiments were made in an irregular order, and not in the order given in the table. Mr. T. Nakashima, a well trained observer, was the subject of the experiments.

TABLE IV.

	A, horizontal.		*B*, frontal.		*C*, median.	
	right	left	right	left	front	back
1	22.5°	22.5°	22.5°	22.5°	22.5°	22.5°
2	45°	45°	45°	45°	45°	45°
3	67°	67°	67°	67°	67°	67°
4	90°	90°	90°	90°	90°	90°
5	112.5°	112.5°	112.5°	112.5°	112.5°	112.5°
6	135°	135°	135°	135°	135°	135°
7	157.5°	157.5°	157.5°	157.5°	157.5°	157.5°
8	0°	180°	0°	180°	0°	180°

The fundamental phenomenon always observed in experiments of this kind is that the two similar impressions received by the two ears were combined into one sound.

The results of these experiments are given in Table V. The table is to be interpreted in the following manner. When the telephones were in the positions given in Table IV, the sound appeared to be in the directions given under similar headings in Table V; thus *A* 1 of the latter corresponds to *A* 1 of the former, etc. The expressions contained in the parentheses represents judgments of this character: "front but a trifle upward," etc. The letter *k* means "in the head;" the other letters have the meanings given on p. 3.

TABLE V.

	A	*B*
1	*f*, *f*(*o*), *f*(*o*), *f*(*ol*), *b*(*ol*)	*k*, *k*(*b*), *k*(*bu*), *k*(*o*), *k*(*fo*), *k*(*bo*)
2	*f*, *f*(*u*), *f*(*o*), *b*, *b*(*k*), *k*(*f*), *k*(*f*)	*k*, *f*(*o*), *k*(*l*), *k*(*b*), *k*(*b*)
3	*b*, *b*, *b*(*k*), *b*(*o*), *k*(*r*)	*k*, *k*, *k*(*u*), *f*(*o*), *f*(*o*)
4	*f*, *f*, *b*, *b*(*k*), *b*(*k*), *b*(*o*), *k*, *k*(*b*)	*f*, *f*, *b*, *b*(*k*), *b*(*k*), *b*(*o*), *k*, *k*(*b*)
5	*b*, *b*, *b*(*k*), *b*(*ko*)	*b*, *b*, *k*, *k*(*b*), *bu*
6	*b*, *b*, *b*, *b*, *b*(*k*)	*b*(*u*), *b*(*k*), *b*(*u*), *b*(*ul*), *b*
7	*b*, *b*, *b*, *b*, *b*(*k*), *b*(*ko*)	*u*(*rb*), *u*(*rb*), *u*(*f*), *u*(*r*), *u*(*rb*)
8	*f*, *f*, *f*, *f*(*o*), *b*(*k*)	*o*, *o*(*b*), *o*(*f*)

	C
1	*f*(*o*), *f*(*o*), *f*(*o*), *f*(*o*), *f*(*o*)
2	*f*(*o*), *f*(*o*), *f*(*o*), *f*, *f*
3	*f*(*o*), *f*(*o*), *f*, *f*, *f*
4	*f*, *f*, *f*, *f*(*o*), *b*(*k*)
5	*b*, *b*(*u*), *f*, *f*, *f*, *f*
6	*b*(*u*), *b*(*u*), *b*, *b*, *b*
7	*u*(*b*), *u*(*b*), *u*(*b*), *b* or *f*, *f* or *b*
8	*o*(*f*), *o*(*b*)

Table V. shows that all the sounds were localized in the median plane. A slight deflection from the median plane, which is indicated by the letters in parentheses, seems to be the effect of slight deviations in the position of the head, in manner of placing the telephones and in occasional difference between the intensities of the two sounds, all of which we could not govern accurately.

In group *A* most sounds were perceived to be at *h* or *h* (*k*). But when the two ears were stimulated by sounds coming from 22.5° and 22.5°, or 45° and 45°, or 0° and 180°, most of the perceived sounds were perceived to be at *f*.

In group *B* the sound was perceived to be at *k* (in the head) when the two ears were stimulated by sounds from above, whereas it was perceived to be at *h* or *u* when the two ears were stimulated by sounds from below. When the sounds were given at *o* and *u* the sound was perceived to be at *o*.

In group *C* most of the perceived sounds were localized at *fo* and *f* when the two ears were stimulated by sounds situated between 0° and 112.5°. But the sounds were mostly perceived at *h* or *u* when the two ears were stimulated by sounds situated lower than 135°.

The conclusion seems to be justified by the results of this set of experiments that, in whichever of the three primary planes the objective sounds may be placed, the perceived sound is always localized (in so far as the sensitiveness of the two ears is the same and the two objective sounds are exactly equal) in the median plane if these sounds are placed in such a way that the distances between one ear and the sources of the sounds are equal to the distances between the other ear and the same sources respectively.

Since equal distances have equal influences upon the intensity of a sound, the above conclusion can be expressed in terms of intensity, namely, when the two ears are stimulated simultaneously by sounds of equal intensity the perceived sound is always localized in the median plane. Conversely we can say that when a sound is localized in the median plane the intensities of the impressions in the two ears are equal.

There still remains the question concerning the component upon which depends the discrimination between front and back, above and below in the median plane. It is true that in the median plane the localization is very imperfect. Still the existence of some localizing power in this plane was proved by the results of the experiments conducted by v. KRIES.[1] Here the localization cannot be explained by the principle of relative

[1] v. KRIES, *Ueber das Erkennen der Schallrichtung*, Zt. f. Psych. u. Physiol. d. Sinn., 1890 I 235.

intensity, for the two ears are stimulated with the same intensity. The experiments of v. KRIES and RAYLEIGH[1] show us that the possibility of the localization in the median plane depends to a great extent upon the constitution of the sound and upon practice. A pure tone, such as that produced by a tuning fork, is in general localized distinctly only with difficulty, but a noise or a tone mixed with overtones (such as the noise produced by striking small blocks against each other, or the human voice) seems to be better localized. The difference which exists between the two cases seems to arise—though it it is not easy to make it definite by an experiment—from the fact that the quality and pitch of a sound are more or less modified according to its position in the median plane, for the sound waves will be more or less influenced by the position of the sound with respect to the position of the pinnæ. Not only the quality and the pitch, but also the absolute intensity of the sound will be different according to its position. These factors will be considered later.

2. *Dependence of the localization of a sound in the horizontal plane upon the unequal intensities of the impressions in the two ears.*

When a source of sound is situated not far from us, either on the left or on the right side, the intensities of the impressions produced in the two ears are not equal, for the intensity of a sound varies (according to the generally accepted law of propagation of sounds) inversely as the square of the distance. The difference between the distances becomes smaller as the source of sound approaches the median plane, while it grows greater as the source of sound moves more toward the side.

The question is whether or not we localize the perceived sound at different points according to the change in the relative difference between the intensities of the sensations received in the two ears. A definite answer was sought by the following set of experiments.

In the preceding experiments I noticed that the reflection of the sound from the surrounding walls had some influence upon the localization. It seemed desirable in further experiments to avoid this source of error, as far as possible. A small separate chamber 4 feet long, 4 feet wide and 4 feet high, with walls of felt, was arranged in a quiet spacious room on the top floor of the Yale Laboratory. Instead of a spherical cage as used in the foregoing experiments the following arrangement was made for determining the objective positions of the telephones.

On the floor of the chamber a circle was described, whose radius was 65^{cm} This circle was divided into 12 equal parts by 12 radii at 30°

[1] RAYLEIGH, *Our perception of the direction of a source of sound*, Nature, 1876 XIV 32.

apart. The person experimented upon was seated in the center of the chamber. His head was adjusted by a support in such a way that the line connecting the openings of the two ears would intersect at its middle point an imaginary line drawn from the center of the circle perpendicularly to the floor. In order to eliminate the influence of suggestion upon judgment, the eyes of the observer were blindfolded before he was allowed to enter the chamber. He consequently never knew anything of its construction or contents. Two telephone-stands of a T shape were prepared. Each stand could be erected on any of the 12 divisions in such away that the longer arm would be perpendicular to the floor and the shorter one would be parallel to the radius at that point. A telephone was hung on the shorter arm by means of strings. The height of the shorter arm was adjusted so that the telephone would lie in the same line with the openings of the ears.

The wires from one telephone were connected with the secondary coil of a sliding inductorium. The wires from the other telephone were connected with the two binding posts at one end of the primary coil, where the electric current coming through an electro-magnetic tuning fork in another room was to be divided into two circuits, one of which served for the primary circuit of the inductorium and the other for the circuit of the second telephone. The current for the latter telephone passed through a copper sulphate rheostat. By means of this rheostat the intensity of the current—and consequently the intensity of the telephone sound—could be regulated. The secondary coil, with which the wires of the first telephone were connected, carried a pointer which passed over the divisions of the millimeter-scale on the base of the inductorium. By changing the distance between the coil the intensity of the induced current—and consequently the intensity of the telephone sound—could be regulated.

In this set of experiments a self-interrupting electro-magnetic fork of 250 complete vibrations was placed as a shunt across the telephone circuit. The current from a lamp battery[1] passed through the fork during half its period of vibration, while during the other half of the period it passed to the telephone apparatus. Thus a tone of 250 vibrations could be produced in the telephones.

The standard intensity of the current could be regulated at pleasure by changing the lamps of the battery. When the telephone circuit was closed by the key each telephone produced a sound of definite pitch, with an intensity depending upon the amount of liquid resistance introduced or upon the distance of the secondary coil from the primary one.

[1] SCRIPTURE, *New apparatus and methods*, Stud. Yale Psych. Lab., 1896 IV 76.

First group.

In the first group of experiments the telephones were placed directly opposite the openings of the ears at a distance of 40^{cm}. The current was turned on for a little more than a second, the two sounds being produced simultaneously. When the sounds ceased the observer was to announce the direction of the sound which he perceived. The experimenter was to take the record both of that direction and of the distance of the secondary coil from the primary, which represented the intensity of the current for one telephone. During the experiment the intensity of the current for the other telephone was kept constant.

The first subject was A. Fisher, the laboratory janitor, a well-trained observer without the slightest knowledge of the arrangements or interest in the results. He always perceived only one sound instead of two separate sounds and projected the sound in one of five directions in a horizontal plane about the same level as his eyes, i. e., at *r*, *fr*, *f*, *fl*, *l*, according to the difference between the intensities of the two sounds. If we call for the sake of convenience the relative intensities "strongest," "stronger," "equal," "weaker" and "weakest," the results can be summarized as follows.

1. When *R*, intensity of the component of the sound on the right side, was "strongest," and *L*, intensity of the component of the sound on the left side, was "weakest," the perceived sound was projected toward *r*.

2. When *R* was "stronger" and *L* "weaker" the perceived sound was projected toward *fr*.

3. When *R* and *L* were "equal" the perceived sound was projected toward *f*.

4. When *R* was "weaker" and *L* "stronger" the perceived sound was projected toward *fl*.

5. When *R* was "weakest" and *L* "strongest" the perceived sound was projected toward *l*.

The next subject was Dr. C. E. Seashore, assistant in the laboratory. He knew where the two telephones were placed and how the intensities would be varied. The results were practically the same as those of the first observer. The only difference was that the second observer could distinguish finer differences of direction than the first observer could. This suggested the possibility of having the results stated in a scale of degrees. This was tried with success on Mr. C. Wakamatsu, a young Japanese student of science, who was totally ignorant of the method of the experiment and the arrangement of the apparatus.

Before stating the results it must be made clear that the exact relation between the intensity of the electric current and the intensity of the telephone sound is not known, and that the rate of change in the intensity of the current which corresponds to the distance of the secondary coil from the primary can not be numerically stated. All we can say is that the intensity of the telephone sound increases with the increase of the induced current and that the intensity of the induced current becomes stronger as the secondary coil is moved nearer to the primary coil. The rate of change in the intensity is not constant; in the particular instrument employed it is rather rapid between 2^{cm} and 4^{cm}, slow between 4^{cm} and 9^{cm} and rapid again beyond 9^{cm}. It must also be noted that we cannot average directly the results for several days; owing to the nature of the experiment we can not keep all the conditions perfectly constant during different days. Slight changes in the positions both of the telephones and the head of the observer and minor errors in apparatus were sufficient to produce somewhat varying results. I am, therefore, compelled in all succeeding experiments to take the average of the results of experiments conducted within a few hours on the same day, during which the above mentioned conditions could be kept tolerably constant. On the present occasion no attempt will be made to establish a numerical relation between the variation in the relative intensities of the two sounds and the variation in the localization of the perceived sound. We must be satisfied if the general dependence of the latter upon the former is proven.

Table VI.

Case I.			Case II.		
Distance of the secondary coil for the left telephone.	Localization.	Number of experiments.	Distance of the secondary coil for the right telephone.	Localization.	Number of experiments.
10^{cm}	*fr* 50°	3	10^{cm}	*fl* 80°	2
9	*fr* 27.5	2	9	*fl* 75	2
8	*fr* 25	2	8	*fl* 60	2
7	*fr* 22.5	2	7	*fl* 41.7	3
6.5	*fr* 18	2	6.5	*bl* 70	2
6	*fr* 10	1	6	*f*	2
5	*fl* 0.5	2	5	*f* or *b*	2
4	*fl* 0.5	2	4	*fr* 60	2
3	*fl* 25	2	3	*fr* 60 or *br* 60	2
2	*fl* 27.5	2	2	*fr* 60	2
1	*fl* 55	2	1	*fr* 60	2
0.5	*fl* 75	1	0.5	*fr* 82.5	2

In Case I the probable error varies from 0 to ± 30%.

In Case II the probable error varies from 0 to ± 8%.

The averages for the experiments are given in Table VI. Case I gives the results when the left sound was varied while the other was kept constant, and Case II gives the results when the right sound was varied and the other was kept constant. In these and subsequent similar experiments, the observer announced the direction of the perceived sound in a scale of degrees, taking *f* and *b* as 0° and counting toward *r* and *l*. For example, *fr* 60° means that the sound is in the front 60° toward *r* and *br* 60° means that the sound is in the rear 60° toward *r*.

To eliminate the effect of suggestion the experiments were made, as in the preceeding section, in an irregular order.

The general relations which were stated on page 18 are shown here more plainly. The transition of the perceived direction according to the change in the relative intensities of the two sounds is not only more gradual, but more minutely scaled. Figure 3 shows diagrammatically the results of the experiments.[1] This figure and the similar figures in the following pages represent the mental field of localization of sound. The median plane of the auditory field coincides with the real median plane of the head, but the mental right and left do not seem to lie exactly in the frontal plane of the head. The directions that appear to us a right and left, seem to lie slightly in front of the auditory axis. The apparent right and left seem to be determined by visual sensations and consequently to lie in a line tangent to both eyes. This seems to afford some explanation of the fact that we localize the preceived sound slightly in the back of the apparent right and left line when the two component sounds have the maximum relative difference in intensity.

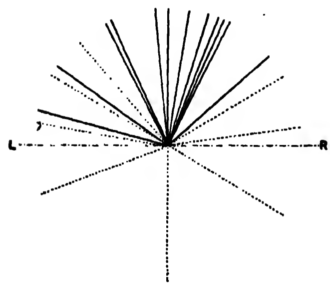


FIG. 3.

In comparing the localizations of Case II with those of Case I we find that the latter is surer and finer than the former. In Case II we find not only a less careful angular scale, but sometimes a bewilderment of judgment as to whether the sound came from the front or from the rear. This difference seems to have its origin in the difference in sensitiveness between the two ears of the observer. The sensitiveness of each ear of the observer was examined by means of the audiometer[2] and it was found

[1] In this and the next figures the full lines represent Case I and the dotted lines Case II.

[2] According to the method described by SCRIPTURE, *Threshold of intensity for sound*, Stud. Yale Psych. Lab., 1896 IV 103.

that the ratio of the sensitiveness of the right ear to that of the left ear was as 10 to 11. It is probable that the discriminating ability for the change in the intensity of a sound depends upon the sensitiveness of the ear. As the right ear of the observer was less sharp than the left ear, the discriminating ability of the former would be less than that of the latter. Accordingly, when the variable soundwas on the left side and the constant sound was on the right side the difference between the intensities of the two sounds would be more accurately perceived than when the sounds were given in the other way.

Moreover, when the variable sound was on the right side and the constant sound on the left side the observer tended sometimes to localize the perceived sound in the rear hemisphere instead of the front, or sometimes he could not decide whether the sound was front or back, though he perceived the angular displacement of the sound from 0°, for example, he could not decide whether the sound came from *f* or *b*, *fr* 60° or *br* 60°. The same uncertainty will be found again in later experiments. The cause of such confusion between front and back location, as we may call it, must be sought in the similarity of the relation between intensities with which the sounds situated in the two directions in question are received by the two ears respectively. This point will receive special consideration later.

TABLE VII.

Case I.			Case II.		
Distance of the secondary coil for the left telephone.	Localization.	Number of experiments.	Distance of the secondary coil for the right telephone.	Localization.	Number of experiments.
12cm	*br* 70°	1	12cm	*l* 90°	1
11	*br* 52.5	2	11		
10	*br* 45	3	10	*bl* 75	3
9	*br* 31	4	9	*bl* 72	3
8	*br* 10	3	8	*bl* 20	3
7	*b*	1	7	*br* 5	3
6.5	*bl* 5	4	6.5	*br* 13	3
6	*bl* 25	4	6	*br* 40	3
5	*bl* 30	3	5	*br* 66.7	3
4	*bl* 57	5	4	*br* 23.3	3
3	*bl* 50	3	3	*br* 83.3	3
2	*l* 90	2	2	*r* 90	3
1	*l* 90	2	1	*r* 90	3

In Case I the probable error varies from 0 to ± 11%.

In Case II the probable error varies from 0 to ± 32%.

We must at any rate conclude that the dependence of the localization upon the relative difference between the intensities of the impressions

in the two ears does not necessitate that the perceived sound will, under the conditions of our experiments, always be localized forward. It is quite reasonable to suppose that the perceived sound will sometimes be localized backward under the same conditions, especially when the intensity of the perceived sound is weakened on account of physical, physiological or mental circumstances. In connection with this the results of experiments upon another person, Mr. W. S. Johnson (a student of psychology), are interesting. He localized all sounds backwards. The average results of the experiments upon him are shown in Table VII. Case I gives the results when the sound of the left telephone was varied, and Case II the results when the sound of the right telephone was varied Figure 4 shows the results diagrammatically.

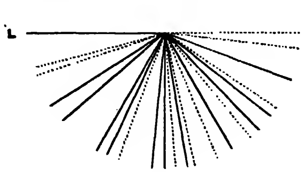


FIG. 4.

The preceding experiments were made without any previous practice, and the observers were required to tell only the direction of the perceived sound. It was found that after some practice an observer could tell not only the direction, but also the distance, of the perceived sound. So the experiments were repeated upon Mr. C. Wakamatsu to determine what would be the apparent distance of the sound. By this time the observer was somewhat practiced in the work, and the judgments were more definitely announced than in the previous experiments. The following table shows the average results. The Japanese unit "sun" was used by the observer in estimating the linear distances. One "sun" is equal to 3.03 cm. nearly.

TABLE VIII.

CASE I.

Distance of the secondary coil for the left telephone.	Judgment of direction.	Judgment of distance.		Number of experiments.
12^{cm}	*fr* 76°	7.3^{sun}	(22^{cm})	3
11	*fr* 67	8.7	(26)	3
10	*fr* 70	7	(21)	3
9	*fr* 27	10	(30)	3
[illegible]	*fl* 12	9.3	(28)	3
7	*fl* 30	8.7	(26)	3
6	*fl* 47	7.7	(23)	3
5	*fl* 70	5	(15)	3
4	*fl* 77	3.7	(11)	3
3	*fl* 80	3.3	(10)	3
2	*fl* 83	3	(9)	3
1	*fl* 83	3	(9)	3

In Case I the probable error for direction varies from 0 to ± 8% and that for distance varies from 0 to ± 3%.

CASE II.

Distance of the secondary coil for the right telephone.	Judgment of direction.	Judgment of distance.		Number of experiments.
12^{cm}	*bl* 85°	8^{sun}	(26^{cm})	2
11	*bl* 82.5	7.7	(23)	3
10	*bl* 55; *bl* 30 or *fl* 30	8	(24)	3
9	*fl* 60; *bl* 40; *fl* 20 or *bl* 20	6 or 8	(18 or 24)	3
8	*b* or *f*	8.7	(26)	3
7	*br* 50	6.3	(19)	3
6	*fr* 65; *br* 50	5.5	(17)	3
5	*fr* 85; *br* 50	4.5 or 5	(14 or 15)	3
4	*fr* 80	4.5	(14)	3
3	*r* 90	3.2	(10)	3
2	*r* 90	3	(9)	2
1	*r* 90	3	(9)	1

In Case II the probable error for direction varies from 0 to ± 3% and that for distance varies from 0 to ± 11%.

Figure 5 shows diagrammatically the results for Case I and Figure 6 the results for Case II.

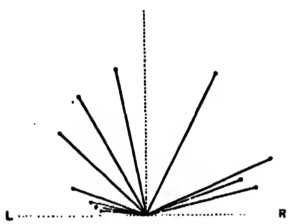


FIG. 5.

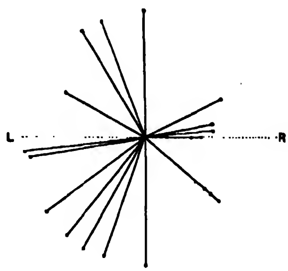


FIG. 6.

The judgment of direction was surer in Case I than in Case II, as in the preceding experiments on the same person. In Case II the sound which was perceived under the same conditions was at one time projected at *f* and another time at *b*, or the sounds which were at one time projected at *fl* 20° and *fl* 30° were at another time projected at *bl* 20° and *bl* 30° respectively.

As to the perception of distance the intensity of the perceived sound seems to furnish the data for the judgments. In the above experiments the intensity of the variable sound was strongest when the secondary coil was at the distance of 1^{cm}; it grew less strong as the distance was greater, and became weakest at 12^{cm}. Accordingly the distance of the sound was perceived as least when the secondary coil was 1^{cm} distant

from the primary, and it was perceived to be greater as the coil was moved towards the other end of the scale. The gradual change in the distance according to the change in the intensity is clearly seen in the results.

The observer seemed to choose a certain intensity as a standard to which he ascribed a certain distance; the distance of another sound seemed to be judged by comparing its intensity with this standard intensity. As a consequence the estimate of distance is different, at least so far as our experiment goes, in different individuals. This can be seen by comparing the above results with the following results which were obtained by making the same experiment on the observer, Mr. W. S. Johnson, in whose case all sounds were, as before, projected towards the back. He estimated the distance in inches (1 inch = $2\frac{1}{2}^{cm}$).

TABLE IX.

CASE I.

Distance of the secondary coil for the left telephone.	Judgment of direction.	Judgment of distance.		Number of experiments.
11^{cm}	*r* 90°	48^{in}	(120^{cm})	2
10	*r* 90	57	(143)	4
9	*br* 52	51	(128)	4
8	*b*	36	(90)	4
7	*bl* 7.5	36	(90)	4
6	*bl* 53	28.5	(71)	4
5	*bl* 75	16.5	(41)	4
4	*l* 90	8	(20)	3
3	*l* 90	6	(15)	1

In Case I the probable error for direction varies from 0 to ± 17% and that for distance varies from 0 to ± 10%.

CASE II.

Distance of the secondary coil for the right telephone.	Judgment of direction.	Judgment of distance.		Number of experiments.
11^{cm}	*l* 90°	60^{in}	(150^{cm})	4
10	*bl* 82.5	57	(143)	4
9	*l* 90	60	(150)	4
8	*b*	39	(98)	4
7	*br* 15	31.5	(79)	4
6	*br* 40	33.3	(83)	4
5	*br* 60	26	(65)	4
4	*br* 82.5	6	(15)	4
3	*r* 90	6	(15)	1

In Case II the probable error for direction varies from 0 to ± 16% and that for distance varies from 0 to ± 9%.

Thus in Johnson's case the greatest distance was 60 inches (150^{cm}), while in the case of Wakamatsu it was about 12 inches (30^{cm}); the shortest distance was 6 inches (15^{cm}) in the case of the former and 3.7 inches (9^{cm}) in the case of the latter.

In the preceding experiments the intensity of the sound was not changed in regular succession, for it was desirable to eliminate the influence of suggestion from the results. It seemed, however, desirable to repeat the experiments by changing the intensity of sound, both descending and ascending in regular order. The average results of the experiments conducted in this way are given in Table X. Here the changes in the direction and the distance were perceived with greater regularity, but in other respects the results were not greatly different from those of the preceding experiments.

Figures 7 and 8 show the results diagrammatically.

In closing the experiments of the first group I tried to see whether a curve of the localized points for the perceived sounds could be obtained if the two sounds were given continuously for a certain length of time during which the intensity of one of the two component sounds was varied in diminuendo or in crescendo. The experiments were made on the observer C. W.

a. When the intensity of the left sound was changed in diminuendo, by sliding the secondary coil from 1^{cm} to 15^{cm}, while keeping the intensity of the right sound constant, the sound was perceived as if travelling

TABLE X.

CASE I.

Distance of the secondary coil for the left telephone.	Judgment of direction.	Judgment of distance.	Number of experiments.
13^{cm}	*r* 90°	8.7^{sun} (26^{cm})	3
12	*fr* 76.7	9 (27)	3
11	*fr* 76.7	9 (27)	3
10	*fr* 73.3	9.3 (28)	3
9	*fr* 61.7	10 (30)	3
8	*fr* 33.3	10 (30)	3
7	*fr* 13.3	9.3 (28)	3
6	*f* or *b*; *fl* 15	8; 9 (24; 27)	3
5	*fl* 26.7	8 (24)	3
4	*fl* 40	6.7 (20)	3
3	*fl* 43.3	5.7 (17)	3
2	*fl* 70	4.5 (13)	3
1	*fl* 70	4.3 (13)	3

In Case I the probable error for direction varies from 0 to ± 25% and that for the distance from 0 to ± 5%.

CASE II.

Distance of the secondary coil for the right telephone.	Judgment of direction.	Judgment of distance.	Number of experiments.
13^{cm}	*bl* 80°	10^{sun} (30^{cm})	3
12	*bl* 76.7	10 (30)	3
11	*bl* 70; *fl* 85	10 (30)	3
10	*fl* 73.3	10 (30)	3
9	*fl* 43.3	10 (30)	3
8	*fl* 30; *bl* 30	10 (30)	3
7	*fr* 35; *f*	9; 8 (27; 24)	3
6	*fr* 43.3	8 (24)	3
5	*fr* 55; *br* 80	7; 5 (21; 15)	3
4	*br* 80	4 (12)	3
3	*fr* 80; *br* 80	4 (12)	3
2	*r* 90	3.7 (11)	3
1	*br* 83.3	3.7 (11)	3

In Case II the probable error for direction varies from 0 to ± 18% and that for distance from 0 to ± 8%.

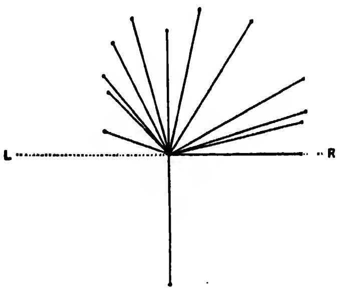


FIG. 7.

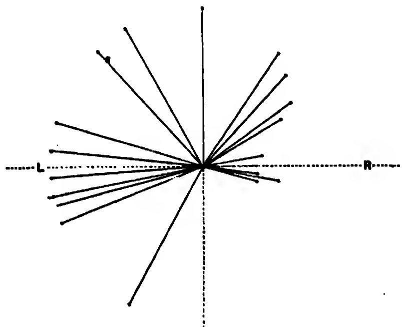


FIG. 8.

from *fl* 80° or *l* 90°, 3^{sun} (9^{cm}), passing to the front at a distance of 6^{sun} (18^{cm}) and stopping at *fr* 80°, or *R* 90°, 12^{sun} (36^{cm}), so that the point of localization described a semi-oval ring with narrow end directed to the right of the observer.

When the intensity of the left sound was varied in crescendo by sliding the secondary coil in the reverse way, the perceived sound started at *fr* 80°, 10^{sun} (30^{cm}), passed to the front at a distance of 7^{sun} (21^{cm}) and stopped at *l* 90°, 4^{sun} (12^{cm}). Sometimes after the sound had reached about *fl* 60° the observer became uncertain whether the sound travelled in front from that point to *l* 90° or travelled in back from *bl* 60° to *l* 90°.

b. When the intensity of the right sound was varied in diminuendo,

and that of the left sound was kept constant, the sound was perceived as if travelling from *r* 90°, 3^{sun} (9^{cm}), passing to the front and stopping at *l* 90°, 8^{sun}, 24^{cm}. The sound seemed sometimes to travel in the rear, though at some points it was uncertain whether the sound was to the rear or the front.

When the intensity of the right sound was varied in crescendo the sound seemed to start at *bl* 80°, 10^{sun} (30^{cm}) and travelling to the rear, to stop at *br* 80°, 4^{sun} (12^{cm}).

This completes the record of the first group. The results show conclusively the existence of a continuous functional relation between the relative difference in intensity (between the impressions in the two ears) and the localization in direction.

Moreover, it can be considered as established that a perceived sound is located on the side from which the stronger sensation is received, the greater the relative difference between the two sensations, the greater being the angular magnitude of the side-localization.

Second group.

When we perceive a sound as situated in the horizontal plane, the intensities of the sensations in the two ears are always different, except when the source of sound is situated in the sagittal, or *fb*, axis. The difference between the two intensities is greatest when the sound is situated nearly in the auditory, or *rl*, axis, for here the difference of distances between the source of the sound and the two ears is greatest.[1] When the source of sound moves gradually from the auditory axis and at the same time approaches the sagittal axis this difference becomes smaller. It has also been shown in the first group of experiments that the localization depends upon the value of the difference between the intensities of the sensations in the two ears. From these two facts it is to be expected that when each ear is affected by two sounds coming from symmetrical points on the two sides respectively the limits within which the perceived sound is localized will be different according to the positions of the symmetrical points.

We have already studied the field of localization of the perceived

[1] It must be noticed here that even when the two ears are equally sharp the difference between the intensities of the sounds heard by the two ears cannot be determined simply by taking the inverse ratio between the squares of the distances from the sound to the ears. When an exact expression is wanted, we must take into account the effects of the refraction of the sound-waves around the head, the reflection of the sound from the surrounding walls, and of the conduction of the sound from one tympanum to the other through the head.

sound when two objective components are placed at *r* and *l*, that is, when the relative difference in intensity between the impressions in the two ears is greatest. In that case the field of localization covered almost an entire semicircle either in front or in back, and sometimes covered more than an entire semicircle. Our next task is to inquire whether this field of localization would be contracted if the relative differences were made smaller. To answer the question, experiments were made on the observer C. W. under the following conditions:

1. Two telephones were situated at *fr* 60° and *fl* 60° respectively.
2. Two telephones were situated at *fr* 30° and *fl* 30° respectively.
3. Two telephones were situated at *br* 60° and *bl* 60° respectively.
4. Two telephones were situated at *br* 30° and *bl* 30° respectively.

The average results of the experiments under condition 1. are shown in Table XI.

TABLE XI.

Case I.				Case II.			
Distance of secondary coil for left telephone.	Judgment of direction.	Judgment of distance.		Distance of secondary coil for right telephone.	Judgment of direction.	Judgment of distance.	
12cm	*fr* 46.7°	10.3sun	(31cm)	12cm	*fl* 36.6°	11sun	(33cm)
11	*fr* 40.8	10.	(30)	11	*fl* 31.7	10.7	(31)
10	*fr* 18.3	11.1	(33)	10	*fl* 23.3	11	(33)
9	*fr* 10	11	(33)	9	*f* 0	11	(33)
8	*fl* 21.7	9.1	(27)	8	*fr* 15.7	9.9	(30)
7	*fl* 46.7	8.4	(25)	7	*fr* 38.3	8.4	(25)
6	*fl* 50.8	6.9	(21)	6	*fr* 48.3	6.9	(21)
5	*fl* 56.6	5.9	(18)	5	*fr* 45	6.3	(19)
4	*fl* 60	4.9	(15)	4	*fr* 48.3	6.4	(19)
3	*fl* 60	4.3	(13)	3	*fr* 48.3	4.6	(14)
2	*fl* 62	3.9	(12)	2	*fr* 51.7	4.3	(13)
1	*fl* 60	3.5	(11)	1	*fr* 48.3	3.8	(11)

The number of experiments on each point is 6.

In Case I the probable error for direction varies from 0 to ± 57%, and that for distance from ± 3% to ± 13%.

In Case II the probable error for direction varies from ± 3% to ± 17% and that for distance from $\frac{8}{10}$% to 8%.

The results are graphically represented in Figures 9 and 10.

Here the relative difference between the intensities of the sensations in the two ears must, for external reasons, have been smaller than that in the preceding experiments. The field of localization of the perceived sound was accordingly much more contracted. When the sound of the left telephone was varied, the field covered a sector included between *fl* 62° and *fr* 46.7°. When the sound of the right telephone was varied, the field was still more contracted, covering a sector included between *fl* 36.6°

and *fr* 51.7°. In both cases most of the perceived sounds were projected on the side on which the source of the variable component sound was situated. Again, in both cases no perceived sound was projected back-

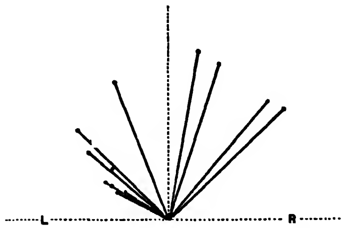


Fig. 9.

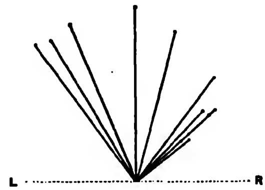


Fig. 10.

ward, and no doubt existed as to the position of the perceived sound, as was the case when two telephones were situated at right and left. The average results of the experiments under condition 2. are given in Table XII.

Table XII.

Case I.			Case II.		
Distance of the secondary coil for the left telephone.	Judgment of direction.	Judgment of distance.	Distance of the secondary coil for the right telephone.	Judgment of direction.	Judgment of distance.
12^{cm}	*fr* 36.7°	11.3^{mm} (34^{cm})	12^{cm}	*fl* 50°	9^{mm} (27^{cm})
11	*fr* 35	11.3 (34)	11	*fl* 46.7	10 (30)
10	*fr* 30	11.3 (34)	10	*fl* 33.3	9.3 (28)
9	*fr* 26.7	11 (33)	9	*fl* 36.7	10 (30)
8	*fr* 20	11.3 (34)	8	*fl* 23.3	9.3 (28)
7	*fr* 7	10 (30)	7.5	*f* 0	10 (30)
6.8	*f* 0	10 (30)	7	*fr* 3.3	9.5 (28)
6	*fl* 16.7	9.3 (28)	6	*fr* 20	9.3 (28)
5	*fl* 16.7	9 (27)	5	*fr* 36.7	8.3 (25)
4	*fl* 30	6.5 (19)	4	*fr* 40	6.3 (19)
3	*fl* 36.7	5.7 (17)	3	*fr* 33.3	6 (18)
2	*fl* 40	5 (15)	2	*fr* 40	4.3 (13)
1	*fl* 40	4.5 (13)	1	*fr* 36.6	4.6 (14)

The number of experiments on each point is 3.

In Case I the probable error for direction varies from 0 to ± 31% and that for distance from 0 to 10%.

In Case II the probable error for direction varies from 0 to ± 19% and that for distance from 0 to ± 10%.

Figures 11 and 12 represent these results graphically.

In Case I the field of localization covered a sector included between *fr* 36.7° and *fl* 40° and in Case II it covered a sector included between

fl 50° and *fr* 40°. The observer was sure of his judgments and never projected the sound backward. The tendency to project the perceived sound more on the side on which the source of the variable sound was situated was lessened here, and the localizations were more evenly distributed.

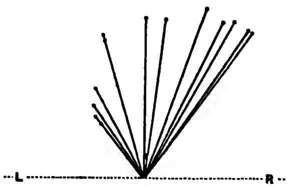


FIG. 11.

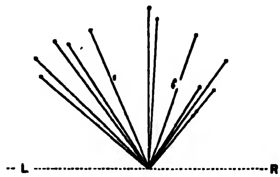


FIG. 12.

A similar contraction of the field of localization of the perceived sound was observed with the same subject when the sources of sound were situated behind the observer. Here we noticed that the perceived sounds were frequently located in front. Under condition 3., when the intensity of the sound on the side of the sharper ear (left) was varied, most of the perceived sounds were located in front, 62 of 72 perceived sounds being decidedly located in front. On the contrary, when the intensity of the sound on the side of the duller ear was varied most of the perceived sounds were located in back, though some of them were located in front. Table XIII gives the average results for the experiments.

TABLE XIII.

CASE I.

Distance of the secondary coil for the left telephone.	Usual localization. Direction.	Usual localization. Distance.		Occasional localization. Direction.	Occasional localization. Distance.	
12cm	*fr* 64°	10sun	(30cm)	*br* 70°	6.5sun	(19cm)
11	*fr* 56	9.4	(28)	*br* 75	7	(21)
10	*fr* 44	10	(30)	*br* 70	6.5	(19)
9	*fr* 40	9.6	(29)	*br* 30	7	(21)
8	*fl* 8.5	10	(30)	*br* 20	6.5	(19)
7	*fl* 31	9	(27)			
6	*fl* 51.6	6.9	(21)			
5	*fl* 60	6.4	(19)	*bl* 40	5	(15)
4	*fl* 66.7	4.5	(14)			
3	*fl* 70	3.8	(11)			
2	*fl* 72	3	(9)	*bl* 80	3	(9)
1	*fl* 80	3	(9)	*bl* 70	3	(9)

The number of experiments on each point is 6.

In Case I the probable error for direction varies from ± 2% to 6% and that for distance from 0 to ± 1%.

CASE II.

Distance of the secondary coil for the right telephone.	Usual localization.		Occasional localization.	
	Direction.	Distance.	Direction.	Distance.
12cm	*bl* 70°	7.3mm (22cm)		
11	*bl* 70	8 (24)		
10	*fl* 45	10 (30)	*bl* 70°	8mm (24cm
9	*bl* 35	7.3 (22)	*fl* 20	10 (30)
8	*f* or *b*	10 (30)	*fr* or *br* 20	8 (24)
7	*fr* or *br* 30	8.5 (25)	*f* or *b*	10 (30)
6	*br* 46.7	6.1 (18)		
5	*br* 46.7	4.3 (13)		
4	*br* 53.3	3.6 (11)		
3	*br* 60	3.3 (10)		
2	*br* 63.3	2.8 (8)		
1	*br* 66.7	3.2 (10)		

In Case II the probable error for direction varies from 0 to ± 12% and that for distance from 0 to ± 6%.

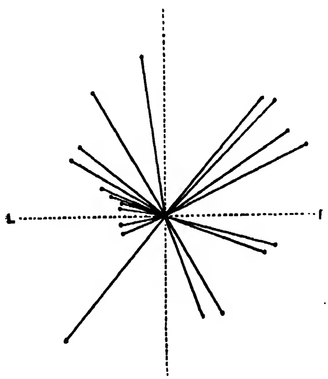


FIG. 13.

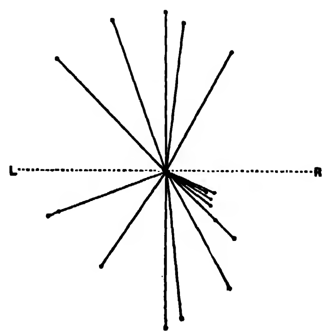


FIG. 14.

Figures 13 and 14 show these results diagrammatically.

The results of the experiments under condition 4 are given in Table XIV.

Figures 15 and 16 show the results graphically.

These results show that the field of localization of the perceived sound covered larger sectors in the experiments under condition 3. than in the experiments under condition 4. In the former it covered a sector included between *fr* 64° and *fl* 80° in front and a sector included between *br* 75° and *bl* 80° in back, while in the latter it covered a sector included between *fr* 55° and *fl* 56.7° in front and a sector included between *br* 60° and *bl* 60° in back.

TABLE XIV.

CASE I.

Distance of the secondary coil for the left telephone.	Usual localization.		Occasional localization.	
	Direction.	Distance.	Direction.	Distance.
12^{cm}	*br* 50°	9^{sun} (27^{cm})		
11	*fr* 35	9 (27)	*br* 50°	8^{sun} (24^{cm})
10	*br* 43.3	8 (24)		
9	*fr* 35	9 (27)	*fr* or *br* 35	9 (27)
8	*fr* 30	9 (27)	*br* 30	8 (24)
7	*f* or *b*	8.7 (26)		
6	*fl* 20	9 (27)	*bl* 30	7 (21)
5	*fl* 43.3	6.8 (20)		
4	*fl* 45	5.5 (16)	*bl* 30	5 (15)
3	*fl* 45	5.5 (16)	*fl* or *bl* 50	5 (15)
2	*fl* 43.3	4.2 (13)		
1	*fl* 45	4.5 (13)	*fl* or *bl* 50	3.5 (10)

The number of experiments on each point is 3.

In Case I the probable error for direction varies from o to ± 65% and that for distance from o to ± 28%.

CASE II.

Distance of the secondary coil for the right telephone.	Usual localization.		Occasional localization.	
	Direction.	Distance.	Direction.	Distance.
12^{cm}	*fl* 40°	8^{sun} (24^{cm})	*bl* 60°	7^{sun} (21^{cm})
11	*fl* 53.3	7.7 (23)		
10	*fl* 56.7	7 (21)		
9	*fl* 50	7.3 (22)		
8	*b* or *k*	7 (21)	*fl* 40 ; *bl* 30	{8 (24) ; 6 (18)}
7	*f*	11 (33)		
6	*fr* 55	6.5 (19)	*br* 40	5 (15)
5	*fr* 45	6 (18)	*br* 40	6 (18)
4	*fr* 45	6.5 (19)	*br* 50	4 (12)
3	*br* 40	4.5 (13)	*fr* 30	4 (12)
2	*fr* 45	4 (12)	*br* 40	3 (9)
1	*fr* 40	3.3 (10)	*br* 50	3 (9)

In Case II the probable error for direction varies from o to ± 8% and that for distance from o to ± 15%.

Our expectation that the acoustic field would be contracted more as the two sources of sounds approached more to the median plane, and consequently the relative difference between the intensities of the sensations in the two ears would grow less, was realized by the second group of experiments. As to the forward and backward projection of the perceived sounds the above results resemble those of the first group of experiments,

where it was asserted that the cause of such forward and backward localization must be sought in the equality of the relation between the intensities with which the sounds localized in the two directions in question are received by the two ears respectively. In a similar way we will say here that the forward and backward projection in the experiments under conditions 3. and 4. originates in the resemblance which exists between the relative differences in the intensities with which the two sounds at the

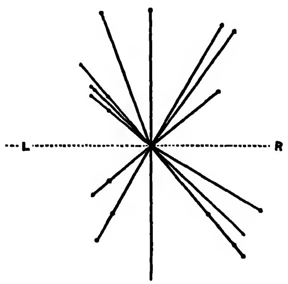


FIG. 15.

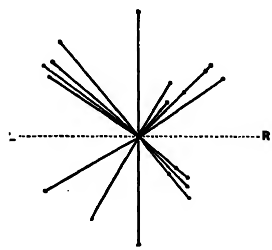


FIG. 16.

back are heard by the two ears and the relative difference in the intensities with which the two sounds at the corresponding points in front are heard by the two ears, for otherwise the sounds would never be localized both in front and in back in such a confused way as shown in the above tables.

3. Previous Investigations.

In going over a large number of monographs that treat of the localization of a sound as depending on the intensities with which the vibratory-movements affect the two ears we find several that are of special importance.

We must first notice SEEBECK's[1] computation of the intensities with which the air-vibrations from a sounding object reach the ears.

Let us suppose that the right ear is turned to a sonorous body and the left toward a wall. Supposing the wave-length of the sound to be λ, the equation of the vibratory movement will be

$$y = a \cos 2\pi \left(\frac{c\,t}{\lambda} + \tau \right),$$

where c represents the velocity of the propagation of the sound, t the time, y the extent to which an air particle is displaced at the moment t,

[1] SEEBECK, *Die Zurückwerfung des Schalles,* Annalen d. Physik u. Chemie (Poggendorff), 1846 LVXIII 465.

a the maximum value of y, and τ a constant time-value indicating the phase. If the time be counted from the moment at which the value for the direct wave is greatest in the right ear, then the vibration will be expressed by

$$y = a \cos 2\pi \frac{ct}{\lambda}.$$

The direct wave reaches the left ear in two ways. The first way is through the air whereby the sound-waves which have proceeded to the head are bent round to the ear. Let d represent the additional distance travelled by the sound-wave in reaching the left ear around the head after having arrived at the right ear. Then the vibration in the left ear will be expressed by

$$y' = -k\, a \cos 2\pi \frac{ct-d}{\lambda},$$

where the minus sign expresses the change in the phase and k is a positive number, which expresses the weakening of the sound due to the refraction around the head.

In addition to the refracted portion the ear receives vibrations transmitted from the right side to the left ear through the solid parts of the head. If $\frac{e}{\lambda}$ be the relation between this route and the wave-length—the latter being referred not to the air but to the solid bony substance, then $\frac{e}{\lambda}$ is a very small quantity for all usual tones. The second part of the wave in the left ear will be expressed by

$$y'' = i \cos 2\pi \frac{ct-e}{\lambda}$$

where i indicates the weakening which the sound suffers on this second route. If we add together these two components for the left ear, we have

$$y''' = -ka \cos 2\pi \frac{ct-d}{\lambda} + ia \cos 2\pi \frac{ct-e}{\lambda} = pa \cos 2\pi \frac{ct-\delta}{\lambda},$$

where
$$p = \sqrt{k^2 + i^2 - 2ik \cos 2\pi \frac{d-e}{\lambda}}$$

and
$$\cos 2\pi \frac{\delta}{\lambda} = \frac{k \cos 2\pi \frac{d}{\lambda} - i \cos 2\pi \frac{l}{\lambda}}{p}.$$

As for the reflected wave, it will be expressed for the left ear by

$$y^{\text{iv}} = -qa \cos 2\pi \frac{ct-l}{\lambda},$$

and for the right ear by

$$y^r = + qa \cos 2\pi \frac{ct - l - \delta}{\lambda},$$

where the factor $-q$ indicates the weakening of the sound and the change in the phase produced by the reflection, and l the distance from the median plane of the head to the wall.

If we now add the elements which proceed from the direct and reflected waves, we obtain for the right ear

$$y^r = a_1 \cos 2\pi \left(\frac{ct}{\lambda} + \tau_1\right)$$

and for the left

$$y^l = a_{11} \cos 2\pi \left(\frac{ct}{\lambda} + \tau_{11}\right),$$

where the phase differences τ_1 and τ_{11} do not concern us for the present purpose, but a_1 and a_{11} are determined by the equations

$$a_1^2 = a^2 \left(1 + p^2q^2 + 2pq \cos 2\pi \frac{2l + \delta}{\lambda}\right)$$

$$a_{11}^2 = a^2 \left(p^2 + q^2 + 2pq \cos 2\pi \frac{2l - \delta}{\lambda}\right).$$

These quantities give the intensities of the two physical sounds in the two ears respectively, for the latter are to be measured by the squares of the amplitudes.

It should be in criticism that the direct transmission of the sound-waves through the skull is an utterly negligible quantity, but that the transmission from one tympanum to the other through the skull is a very important, though undeterminable, factor.

We must next consider STEINHAUSER's theory of binaural audition.[1] He worked out on geometrical principles the laws which determine the relative intensities with which a sound will reach the two ears. According to him the pinna acts as a funnel to conduct into the ear those waves of sound which in consequence of their direction reach but could not otherwise enter it.

The regions of direct, indirect, and mixed hearing were distinguished according to the nature of the path by which the sound waves reach the

[1] STEINHAUSER, *The theory of binaural audition*, Phil. Mag., 1879 (5) VII 181, 261. (This is a translation of STEINHAUSER, *Die Theorie d. binauralen Hörens*, Wien 1877.)

ears. In direct hearing the waves proceeding from the sonorous body reach the ear in a straight line and enter the auditory meatus directly. In indirect hearing the waves proceeding from the source of sound do not reach the ear in straight lines, but after undergoing reflection from external objects. In mixed hearing the waves reach the ear partly directly and partly indirectly.

The formula deduced by STEINHAUSER for direct hearing is as follows: Let β be the angle between the plane of the pinna and the line of sight, and α the angle which the line of sight makes with the direction of the sound; and let i_1 and i_2 represent the relative intensities with which the sound is heard in the two ears; then

$$\tan \alpha : \tan \beta :: i_1 - i_2 : i_1 + i_2 .$$

The direction in which a source of sound is situated may, therefore, be, according to STEINHAUSER, estimated by the different intensities with which a sound is perceived in the two ears. From the above formula the case can be deduced, in which $i_1 = i_2$. We have then $\tan \alpha = 0$ and hence $\alpha = 0$. This is the case in which the sound is just in the direction of the line of sight.

If a source of sound is situated in the region of indirect hearing, no waves of sound can reach the surface of either of the pinnæ directly; the sound produced by the sonorous body can evoke a sensation as the results of reflection, provided we neglect the possible conduction of sound through solid bodies and by refraction around the head and pinnæ. Let α be the angle which the rays of sound make with the line of sight before reflection, α_r the angle they make after reflection, and φ be the complementary of the angle which the line of sight makes with the surface which reflects the rays of sound; then by a geometrical operation

$$\alpha_r = 2\varphi - \alpha.$$

That which is heard, therefore, indirectly in the direction α makes the same impression as that heard directly in the direction α_r; in which case α_r, whose value is dependent on φ, may, without any change of the direction of the waves of sound, assume an indefinite number of different values, since the position of the reflecting surface may as well be any other than it is, or there may be many reflecting surfaces.

If a source of sound is situated in the region of mixed hearing, then the direct rays of sound can reach only one of the two pinnæ, while both may be reached by the indirect rays. Accordingly let i_1 be the intensity with which the direct rays of sound affect one ear, and ρ_1 the increment of that intensity due to the effect of reflexion. Let ρ_2 be the

intensity of the sensation in the right ear, due to the reflexion alone. Then, on summing up the indirect and direct effects, the following equation is obtained:

$$\tan \alpha = \frac{i_1 + \rho_1 - \rho_2}{i_1 + \rho_1 + \rho_2} \tan \beta.$$

Hence, by calculation, we can find the angle within the region of direct hearing in which the source of sound is erroneously imagined to lie.

STEINHAUSER devised an instrument called homophone, wherewith to test his theory. It consisted of a system of wooden tubes for bringing to the ears the sounds of two organ-pipes tuned to unison, whose respective intensities could be regulated by stop cocks. It was held by its inventor to confirm his theory.

The observation that by binaural audition the image of the perceived sound is localized apparently in the occipital region of the head was made first by PURKYNÉ.[1] In his experiments it was shown that when a tone was conducted simultaneously to the two ears separately by means of rubber tubes the acoustic image was not perceived in the ears, but was perceived in the occipital region of the interior of the head.

An apparently similar localization of the sound in the occipital or frontal region was observed by several subsequent investigators, such as S. P. THOMPSON,[2] PLUMAUDON,[3] URBANTSCHITSCH,[4] and KESSEL.[5]

POLITZER[6] emphasized the fact that for the perception of the direction of a sound the fact of binaural hearing was requisite. He made numerous experiments upon normal and abnormal persons and found that in monaural audition the sound was localized on the side of the open ear. When a watch was moved in the horizontal plane and heard with one ear closed, the tick-tick was localized on the side of the open ear even when the watch was moved some distance farther to the other side of the median plane. The perception of the position of the sound became more difficult when the sound was moved further toward the closed ear. In the case of persons who suffered from diseases of the ears a similar error was observed, a mistake of 180° for perceiving the direction of a sound being often made. The diminution of the localizing power was

---

[1] PURKYNÉ, Prager Vierteljahresschrift, 1860 III 94.

[2] THOMPSON, *On binaural audition*, Phil. Mag., 1877 (5) IV 274; 1877 (5) VI 283.

[3] PLUMAUDON, The Telegraphic Journal, London, Sept. 1879.

[4] URBANTSCHITSCH, *Zur Lehre von der Schallempfindung*, Arch. f. d. ges. Physiol. (Pflüger), 1881 XXIV 574.

[5] KESSEL, *Ueber die Function der Ohrmuschel bei den Raumwahrnehmungen*, Arch. f. Ohrenheilk., 1882 XVIII 120.

[6] POLITZER, *Studien über die Paracusis loci*, Arch. f. Ohrenheil., 1876 XI 231.

observed also in persons who suffered from hard hearing of one or both ears. With many persons the mistakes of localization were first noticed when they were subjected to a special test. He ascertained also that in binaural audition the localization was especially defective for a sound in the median plane, where the source of sound was equally distant from the two ears.

THOMPSON noticed, during his experiments on binaural hearing, an acoustic illusion due to the fatigue of the ear. One ear was fatigued by listening to a loud pure tone, and then the listener tried to estimate the direction of a sound of the same pitch. If his left ear were fatigued he would invariably imagine the source of the sound to be further to the right than it really was, and likewise the reverse. The illusory displacement in the direction of the sound was greater as the fatigue was more complete. But as the sounds of different pitch have to stimulate different fibres of the basilar membrane of the cochlea, it would be expected that the fatigue produced by a sound of a certain pitch would have no effect on the perception of a sound of another pitch. According to THOMPSON'S experiments, when one ear was fatigued with a *c''* fork no illusory displacement was perceived in an *a''* fork.

TARCHANOFF[1] found that when telephones were held opposite the ears and intermittent currents were sent through them, the perceived sound was localized in the median plane and that it was perceived outside the plane when there was the slightest difference between the intensities of of the two sounds.

URBANTSCHITSCH[2] found that when one and the same sound of a certain intensity was led into the two ears separately by means of a T tube, one group of his observers perceived the sounds as being in the right and left ears with equal intensities, whereas another group perceived the sounds in the right and the left temporal regions of the head. When the intensity increased, these two sounds seemed to expand and approached nearer to the middle of the head, being finally brought into fusion at a sufficient intensity. Sometimes besides the two sounds a third sound was observed in the center of the head; the latter was perceived after repeated experiments or only when the observers paid special attention to it. A large proportion of the observers, when very attentive, perceived the sound—not in the ears but in the interior of the head. Finally, there were some observers by whom the sound was not generally located in the head, but projected in front, to the back, or above.

[1] TARCHANOFF, St. Petersburger med. Wochenschrift, 1878, No. 43.

[2] URBANTSCHITSCH, *Zur Lehre von der Schallempfindung*, Archiv f. d. ges. Physiol. (Pflüger), 1881 XXIV 574.

In regard to the intercranial localization there were some individual differences as to the distinct position of the perceived sound. Some localized it in the occipital region of the head, others in the frontal region or in the forehead, while still others localized it in the nose or in the pharynx. URBANTSCHITSCH asserted also that the degree of fatigue of the ear could possibly be determined by observing the position of an acoustic image. At first a very strong sound was conducted to one ear for a while; then weaker sound of the same pitch was conducted to the two ears simultaneously. The acoustic image for the latter case was found at first in the ear which was not fatigued and remained there for a few seconds; then the image travelled gradually towards the median plane and at last was found at the centre of the head. URBANTSCHITSCH explained the phenomenon in the following way. As one ear was very strongly stimulated in the beginning it was fatigued for some interval of time and the other ear, which was not stimulated, became relatively sharper. Consequently, when a weaker sound was conducted to the two ears simultaneously the sensation in the latter ear was evidently stronger and the perceived sound was located on the side of that ear. But the other ear began to recover gradually, and at the same time the sharper ear began to be fatigued. As a result, the perceived sound began to travel towards the median plane and at last reached the center of the head when the sharpness of the two ears became the same.

KESSEL[1] performed his experiments by inserting tubes from a funnel shaped sound-receiver into the ears, thus excluding the action of the pinnæ. The sound of a tuning fork was conducted into the two ears by this apparatus. When the tubes were of the same length and were opened equally wide the ears were stimulated with the same strength and the resulting single sound was located in the median plane of the head. If one of the tubes was more or less pressed so that the two ears were stimulated differently, then the sound was perceived in the ear which was stimulated more strongly.

KESSEL made another interesting experiment. According to him the principle of the localization of a sound on the side of a stronger excitation holds good even in the case when one ear is not directly stimulated by the waves of the sound, but stimulated by reflected waves. To prove this he hung a watch in the axis of a parabolic mirror; the head of the observer was adjusted between the mirror and the watch so that the two ears were opposite them. Then the watch was so adjusted that the reflected rays, which were gathered at the focus, would stimulate the ear oppo-

[1] KESSEL, *Ueber die Function der Ohrmuschel bei den Raumwahrnehmungen*, Arch. f. Ohrenheilk., 1882 XVIII 120.

site the focus more strongly, in which case the sound was localized in the direction of the mirror. If the watch was brought nearer to the ear on its side, so that the direct rays of the sound would stimulate this ear more strongly, then the sound would be localized on the side of the watch.

ROGDESTWENSKY[1], one of TARCHANOFF's pupils, produced sounds at symmetrical points, and observed that the perceived sounds were localized in the head, breast and abdomen according to the difference in the height of the symmetrical points.

To exclude the function of one ear in perceiving the direction of a sound, PREYER[2] made experiments by using a telephone instead of the snapper sounder. The intensity of the telephone sound was weakened by reducing the intensity of the electric current so that by stopping the ears the sound could not be heard by the subject. Then one ear was opened so that the sound was heard by that ear only. The result of the experiments showed that under such conditions errors occurred which were not observed in ordinary perception, and it was often very difficult, even with a strenuous attention, to get rid of these errors. These errors were localisations on the wrong side.

Similar experiments were afterwards made by ARNHEIM[3]. In his experiments the number of correct perceptions with one ear amounted to only 22% of the total, while with both ears it amounted to 39.7% (in PREYER's experiments 30%). He noticed also a decided tendency to locate the perceived sound on the side of the open ear.

SCHAEFER,[4] who had made elaborate experiments on the perception of the direction of sound in coöperation with PREYER, attacked a peculiar side of the problem at a later date, namely, the localization of beats and difference tones. His results may be summarized as follows.

If the relative intensity of the primary tones is equal, then the beats appear to proceed from the region between the points at which the two tones are situated whether they are on the same or different sides of the median plane. The localization of the beats in the median plane when the primary tones of equal intensity are situated on both sides of the plane, is a special case of this general fact. SCHAEFER thinks it clear that when the two forks are placed on the same side of the median plane the

---

[1] ROGDESTWENSKY, *Ueber die Localisation der Gehörsempfindungen*, Diss. 1887.

[2] PREYER, *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Archiv f. d. ges. Physiol. (Pflüger), 1887 XL 586.

[3] ARNHEIM, *Beiträge zur Theorie von Schallempfindungen mittelest der Bogengänge*, Diss., Jena 1887.

[4] SCHAEFER, *Ueber die Wahrnehmung und Localisation von Schwebungen und Differenztönen*, Zt. f. Psych. u. Physiol., 1890 I 81.

beats are perceived more strongly by the ear on that side; and that the beats are perceived by the two ears with equal intensity when the sources of the tones are found in the median plane. For sounds located on different sides of the median plane it can be, according to SCHAEFER, mathematically proven that the intensity of the beats is equal on the two sides when the relative intensities of the primary tones are equal, but on the contrary the intensity of the beats is stronger on the side of the stronger primary tone when the relative intensities of the primary tones are not equal. SCHAEFER'S conclusion is that the beats will be localized on the side of the ear which is more strongly excited by them, but in the median plane if the two ears are equally excited by them. The further exact determination of the direction is dependent upon that of the relatively stronger primary tone. The localization of beats is, therefore, governed by the same principles as the localization of simpler sounds, i. e., the localization on the side of the ear which is excited more strongly, and the localization of sounds in the median plane when the two ears are excited with equal intensity.

As for the perception of difference tones, the localization is apparently contradicted by that of beats. For when two forks of unequal intensities are placed on the different sides of the median plane the difference tones are heard on the side of the weaker primary tone. This is not, however, really contradicted by the localization of the beats on the side of the stronger primary tone. For the localization of difference tones on the side of the weaker primary tone is based upon the fact that on this side the relation of intensity which is more favorable for the perception of the difference tones predominates, and the difference tones are heard louder on this side, for if the left sound, for example, is relatively stronger, the left ear is made "physiologically deaf" for the sound coming from the right side, and thereby the perception of the difference tone is made impossible. Difference tones are, therefore, localized after all on the side of the ear which is excited more strongly. As for the median localization of the difference tones, the result is similar to that of the beats, for when the two primary tones of equal intensity reach the ears from the two sides of the median plane either by air transmission or by cranial conduction, the difference tones are localized in the median plane.

Finally, one more phenomenon which was emphasized by SCHAEFER[1] is to be mentioned. If a fork be placed on the top of the head the sound will be localized in the median plane, but it will shift to one ear if that ear be closed. SCHAEFER explained this phenomenon

[1] SCHAEFER, *Ein Versuch über die intrakranielle Leitungleises tar Töne von Ohr zu Ohr*, Zt. f. Psych. u. Physiol. d. Sinn, 1891 II 111.

in the following way. The osseous parts of the auditory apparatus, the tympanum, and finally the air in the external auditory tube will be put into vibration by the waves which the sound produces in the labyrinth by means of cranial conduction. In this case, therefore, sound proceeds in just the opposite way to the usual one, where it passes from the air into the ear. Now, if we close the ear with the finger, the tympanum will be put into stronger vibration on account of the reflexion of the waves from the finger. Consequently the component sound is stronger in the closed ear and the perceived sound will appear to shift from the median plane toward the side of the closed ear. The same effect will be produced if we apply a resonator to the ear either by holding by fingers or by supporting on a stand. To this same class belongs another phenomenon which is noticed by many persons of normal hearing. When we sing loudly a low tone like the German "u" and stop one ear, but not very tightly, the tone will move from the initial position in the larynx to the stopped ear, but it will move again to the median plane in the interior of the head if the other ear is stopped in the same way.

Bloch [1] investigated both binaural and monaural localization by measuring the least noticeable change in the position of sounding body. His conclusion runs as follows.

The most important function of binaural audition is the perception of the direction of a sound; the perception of direction is more accurate in the horizontal and in the frontal planes than in the median plane; in the former two planes the localization is based chiefly upon the relative difference between the intensities of the sounds heard by the two ears, and, secondarily, upon the change in the intensity of the perceived sound which arises from the influence of the pinnæ; and in the median plane the action of the pinnæ which collect the sound-waves into the auditory meatus is the chief condition for the localization of sounds.

These investigations make it clear that the relation of intensity between the two components of a sound heard by the two ears is a fundamental—or the fundamental—factor of localization in regard to direction. My own work, reported in this section, aimed to further define this factor and its effect.

III. Localization of the Perceived Sound at the Middle Point between the Sources of Two Objective Sounds.

In the foregoing experiments the two telephones were restricted to the same primary plane at two points.

[1] Bloch, *Das binaurale Hören*, Zt. f. Ohrenheilk., 1893 XXIV 25.

In the following experiments the telephones were placed at various points on the surface of the spherical cage (Fig. 1).

(*a*) One of two telephones was situated in the median plane and the other in the frontal plane. The two telephones were placed at a same level. The physical intensities of the two sounds were kept as far as possible equal and constant during the experiments. The positions of the two telephones were as follows:

TABLE XV.

Number of position.	Position of the two telephones
1	*fo* and *ro*
2	*fo* " *lo*
3	*fu* " *ru*
4	*fu* " *lu*
5	*bo* " *ro*
6	*bo* " *lo*
7	*bu* " *ru*
8	*bu* " *lu*
9	*f* " *r*
10	*f* " *l*
11	*b* " *r*
12	*b* " *l*

On the one hand, if a sound comes from the median plane only, the two ears will be stimulated equally, and consequently the perceived sound will be located in the median plane. On the other hand, if a sound comes from the frontal plane only, the two ears will be stimulated with the maximum relative difference in intensity, so far as the sounds at a same level are concerned, and consequently the perceived sound will be located nearly in the frontal plane. Now, if the two ears are stimulated simultaneously, as in our experiments by the two sounds situated in the above two planes, the relation between the intensities of the sounds heard by the two ears will be just like the relation between the intensities with which the two ears hear a sound coming from the plane which lies just between the above two planes. The perceived sound may accordingly be expected to be located in the plane between the median and frontal planes. Moreover, in our experiments, as the two telephones lie on the same level the perceived sound may be expected to be mostly localized on that level.

These expectations were fully realized by the actual results which are given in Table XVI. Mr. K. Matsumoto, a graduate student of psychology, and Mr. T. Nakashima were the subjects of the experiments.

To make the comparison between expectation and realization easier, I have arranged the results as in Table XVII. The symbols in

TABLE XVI.

Localization.

	Observer T. N.	Observer K. M.
1	*for, for, for, fo(r), o(r), fur.*	*for, (f)or, o(r) o, o(b), bor (k).*
2	*fol, fol, fol, o, o(b), b(o)l.*	*fol, ol, ol, ol, o(b), o.*
3	*fur, fur, fur, fur, (f)ur, bor (k).*	*fur, fur, bor, bor, b(u)r.*
4	*ful, ful, ful, ful, ful, ul, b(u)l.*	*ful, ful, bol, bo(l), bol.*
5	*bor, bor, for, for, fo(r), fur.*	*bor, bor (k), bor (k), for, fo, o(k).*
6	*bol, bol, b, o, ful, ful, (f)ul.*	*bol, bo, bo, lo, o, bo, fol.*
7	*fur, fur, fur, fur, ur.*	*bur, bur, bur, bur, (b)ur, hor, hor, ho, ur.*
8	*bul, bul, (b)ul, ul, (f)ul.*	*bul, bul, (b)ul, ul, bol, bol.*
9	*fr, fr, fr, f(k), fo(r)(k), b(r).*	*bor (k), bor (k), bor.*
10	*fl, fl, fl(u), bol, br, br, br, bor, bor.*	
11	*b(k), b* or *f, bl, l, b, bol(k), bol.*	
12	*fl.*	

TABLE XVII.

	A Expected localization.	*B* Usual localization	*C* Occasional localization.	*D* Rare localization.
1	*for*	*for*	*bor, bur*	
2	*fol*	*fol*	*bol*	
3	*fur*	*fur*	*bur, bor*	
4	*ful*	*ful*	*bul, bol*	
5	*bor*	*bor*	*for*	
6	*bol*	*bol*	*ful*	
7	*bur*	*bur*	*fur*	
8	*bul*	*bul*	*bol*	
9	*fr*	*fr*		
10	*fl*	*fl*		*for, bor*
11	*br*	*br*		*bol*
12	*bl*	*bl*	*fl*	*bor*

column *A* show the directions in which the perceived sounds are theoretically expected to be localized; the symbols in column *B* the directions in which most of the perceived sounds were actually localized, and

the symbols in columns *C* and *D* the directions in which the perceived sounds were sometimes localized. The striking correspondence between *A* and *B* proves the correctness of our view. It is also very interesting to note that the perceived sounds were sometimes located in the directions in column *C* instead of the directions in column *B*. There is a reasonable justification for such localizations. When we are stimulated by a sound from the median plane the intensities of the sounds heard by the two ears are equal whether the sound is situated in front or in back. Subjective discrimination between front and back is, as we will see later, based chiefly upon the difference in absolute intensity, for the sound coming from the front is heard more strongly than the same sound from the back. So if the sound from the front in the median plane be weakened during the experiment by fluctuation of the electric current, while the ears are stimulated at the same time by the sound in the frontal plane, it will be quite possible that the relation of intensities will be like the relation of intensities with which the observer hears the sounds from the frontal plane and from the back part of the median plane. In such a case the observer may locate the sound to the rear instead of locating it to the front. This appears to be the reason why in the above experiments the perceived sounds were sometimes located at *bor* or *bol* instead of *for* or *fol*; at *bur* or *bul* instead of *fur* or *ful*. For the similar reasons the perceived sounds were located at *ful* or *fur* instead of *bul* or *bur*; and at *fl* instead of *bl*. These results agree with the confusion between front and back which we have frequently observed in our previous experiments.

Besides the confusion between front and back we find here another kind of confusion; above and below are sometimes confused with each other. In the above experiments the observer located the sound at *fur* when *for* was expected; at *bor* or *bol* when *bur* or *bul* was expected. This kind of confusion can be explained by the function of the pinnæ. It is found that on account of the peculiar shape of the pinnæ a sound coming from *ro* or *lo* is perceived by the two ears with almost the same relative difference of intensity as a sound coming from *ru* or *lu* and a sound coming from *fu* is perceived with almost the same intensity as a sound coming from *fo*. So when we are stimulated simultaneously by sounds coming from *fo* and *ro* it is quite possible that the relation between the intensities of the sounds in the two ears will be like the relation between the intensities with which the sounds coming from *fu* and *ru* are heard by the two ears. In such a case the perceived sound may be located at *fur* instead of *for*. Moreover, the fluctuation in the electrical current will have some effect in producing the confusion. Such being the

fact, we have reasonable justification, under the above conditions, for occasionally localizing the perceived sounds in the directions in column *C*.

As for the localizations of the sounds in the directions in column *D*, they occurred very rarely and could be ascribed both to the inaccuracy of the experiment and inattention of the observer.

(*b*) The localization of the perceived sound at the middle point between the sources of the two component sounds is not restricted to the case in which the components sounds are put in the primary planes and at the same level. In the following experiments I placed two component sounds at different levels and one of them in a secondary plane, namely, one telephone was placed at the terminus of a secondary axis in the horizontal plane and the other telephone was placed at the terminus of a secondary axis in the frontal or median plane. The objective positions of the telephones were, therefore, as indicated in Table XVIII.

TABLE XVIII.

Number of position.	Positions of the two telephones.	Number of position.	Positions of the two telephones.
1	*fr* and *ru*	9	*fr* and *ro*
2	*r* " *fu*	10	*fr* " *fo*
3	*fl* " *lu*	11	*fl* " *lo*
4	*fl* " *fu*	12	*fl* " *fo*
5	*br* " *ru*	13	*br* " *ro*
6	*br* " *bu*	14	*br* " *bo*
7	*bl* " *lu*	15	*bl* " *lo*
8	*bl* " *bu*	16	*bl* " *bo*

The experiments were made upon T. N. and the results were as given in Table XIX.

TABLE XIX.

	Localization.		Localization.
1	*fr(u)*, *fr*, *fr*, *fr*, *fr*, *fr*, *fr*.	9	*f(o)r*, *f(o)r*, *f(o)r*, *fr*, *fr*, *fr*, *fr*.
2	*fr(u)*, *bur(k)*, *fr*, *fr*, *fr*, *fr*, *fr*.	10	*fo(r)*, *fo(r)*, *f(or)*, *f(r)*, *fr*.
3	*fl(u)*, *fl(u)*, *fl(u)*, *fl(u)* *fl(u)*, *fl*.	11	*f(u)l*, *f(u)l*, *l(u)*, *fl*, *fl*, *fl*.
4	*fl(u)*, *fl(u)*, *fl(u)*, *fl*, *fl*, *fl*.	12	*f(u)l*, *fl(k)*, *(f)l*, *o(k)*, *fl*, *fl*.
5	*br(u)*, *ru(b)*, *ru(b)*, *br*, *br*, *r(b)*.	13	*(b)ur*, *(b)ur*, *(b)ur*, *f(u)r*, *bur*, *br*, *br*.
6	*bur*, *bur*, *(b)ur*, *b(u)r*, *br(k)*, *br*.	14	*b(u)r*, *bu(k)*, *b(uk)*, *b(k)*, *b*, *b* or *f*.
7	*bul*, *bul*, *bul*, *b(u)l*, *bl*.	15	*(b)ul*, *(f)ul*, *lu*, *lu*, *l*.
8	*bul*, *bul*, *bul*, *bul*, *bul(k)*, *b(l)*.	16	*b(u)k*, *bu*, *b(k)*, *f*, *f(k)*, *b(k)*, *b(k)*.

As the two telephones were situated at different levels, the localization of the perceived sound at the middle point between the sources of the two objective sounds was not so clear as in the preceding experiments. Still the results were in conformity with the preceding results, for the perceived sound was localized or tended to be localized at the middle point.

I have already mentioned that a sound coming from *ro*, *lo*, *fo* or *bo* is perceived by the two ears with almost the same intensity as a sound coming from *ru*, *lu*, *fu* or *bu* respectively. This is the reason why the perceived sound was sometimes located under the above conditions at a point on the same side at 90° away from the middle point between the sources of the two component sounds, i. e., at the corresponding point in back instead of front, above instead of below and vice versa.

(*c*) In the preceding two groups of experiments the two objective sources of sounds were kept unmoved during the experiment. If the perceived sound is located under such a statical condition at the middle point between the two objective sounds it may also be expected that if the two sounds are moved continuously during the experiment the perceived sound will move, too, in the direction resultant to the two directions along which the two sounds are moved.

TABLE XX.

	Directions in which the two telephones were moved.		Directions in which the two telephones were moved.
1	$fl\left\{\begin{matrix}ful\\f\end{matrix}\right.$	13	$fr\left\{\begin{matrix}for\\f\end{matrix}\right.$
2	$fl\left\{\begin{matrix}ful\\l\end{matrix}\right.$	14	$fr\left\{\begin{matrix}for\\r\end{matrix}\right.$
3	$bl\left\{\begin{matrix}bul\\b\end{matrix}\right.$	15	$br\left\{\begin{matrix}bor\\b\end{matrix}\right.$
4	$bl\left\{\begin{matrix}bul\\l\end{matrix}\right.$	16	$br\left\{\begin{matrix}bor\\r\end{matrix}\right.$
5	$fr\left\{\begin{matrix}fur\\f\end{matrix}\right.$	17	$f\left\{\begin{matrix}fr\\fo\end{matrix}\right.$
6	$fr\left\{\begin{matrix}fur\\r\end{matrix}\right.$	18	$f\left\{\begin{matrix}fl\\fo\end{matrix}\right.$
7	$br\left\{\begin{matrix}bur\\b\end{matrix}\right.$	19	$b\left\{\begin{matrix}br\\bo\end{matrix}\right.$
8	$br\left\{\begin{matrix}bur\\r\end{matrix}\right.$	20	$b\left\{\begin{matrix}bl\\bo\end{matrix}\right.$
9	$f\left\{\begin{matrix}fr\\fu\end{matrix}\right.$	21	$fl\left\{\begin{matrix}fol\\f\end{matrix}\right.$
10	$f\left\{\begin{matrix}fl\\fu\end{matrix}\right.$	22	$fl\left\{\begin{matrix}fl\\l\end{matrix}\right.$
11	$b\left\{\begin{matrix}br\\bu\end{matrix}\right.$	23	$bl\left\{\begin{matrix}bol\\b\end{matrix}\right.$
12	$b\left\{\begin{matrix}bl\\bu\end{matrix}\right.$	24	$bl\left\{\begin{matrix}bol\\l\end{matrix}\right.$

This was the subject of the next experiments. As is show in Table XX the two telephones were started from one and the same point of the horizontal circle, and then one telephone was moved along the horizontal circle while the other was moved along one of the two vertical circles downward or upward. For example, the telephones were started at *fl* and one was moved toward *ful* while the other was moved toward *f*. The experiments were made upon T. N.

Table XXI gives the results of the experiments. The expressions indicate the directions in which the sound appeared to move; for example, *fl–fu* indicates that the sound appeared to move from *fl* to *fu*.

TABLE XXI.

	Direction in which the perceived sound moved.		Direction in which the perceived sound moved.
1	*fl–fu, fl–fu, fl–fo, fl–f, fl–ful, fl–ful.*	13	*fr–fo, fr–fu, fr–f, fr–f, fr–f, fr–for.*
2	*fl–lu, fl–lu, fl–l(u), fl–lo, fl–l.*	14	*fr–ro, fr–ro, fr–r(u), fr–r, fr–r, fr–r(k).*
3	*bl–bu, bl–bu, bl–bu, bl–bu, bl–b.*	15	*br–bu, br–b, br–b(k), br–b(k), br–b(k).*
4	*bl–lu, bl–lu, bl–l, bl–l, bl–l.*	16	*br–ru, br–u* or *br–r, br–r, br–r, br–r.*
5	*fr–fu, fr–fu, fr–fu, fr–fur, fr–fur, fr–fur.*	17	*f–fur, f–fur, f–fur, f–fr, f–fr, f–fr(k).*
6	*fr–ru, fr–ru, fr–ru, fr–ru, fr–ru.*	18	*f–fol, f–ful, f–fl, f–fl, o–l(k), o–fo(k).*
7	*br–bu, br–bu, br–bu, br–bu, br–bu.*	19	*b–bur, b–bur, bo–bur, bo–bur, bo–b.*
8	*br–ru, br–ru, br–ru, br–ru, br–r.*	20	*b–bul, b–bul, b–b(u)l, b–b(u)l.*
9	*f–fur, f–fur, f–fur, f–fur, f–fr.*	21	*fl–fu, fl–lo, fl–lo, fl–lo, fl–f.*
10	*f–ful, f–ful, f–ful, f–f(u)l, f–fl.*	22	*fl–lo, fl–lo(k), fl–fu, fl–l, fl–l.*
11	*b(u)–bur, b–bur, b–bur, bu–br, b–br.*	23	*bl–bo, bl–bu, bl–bu, bl–b.*
12	*b–bul, b–bul, bu–bl, b–bl.*	24	*bl–lu, bl–lu, bl–lu, bl–l, bl–l, bl–l.*

When one of the two telephones was moved along the horizontal circle and the other was moved downward (i. e., cases 1 to 12) the sound appeared to move in a direction resultant to those directions along which the two telephones were moved. The results can be explained by the relative and absolute differences in the intensities of the sounds in two the ears. If one telephone moving from *fl, fr, bl* or *br* to *f* or *b* as in 1, 3, 5, 7 were to act alone the relative difference in the intensities of the sounds in the

two ears would decrease gradually, and consequently the sound would be perceived to move from the secondary vertical plane towards the sagittal plane. On the other hand, if the other telephone moving downward were to act alone, not only the absolute intensity of the sound, but also the relative difference between the intensities of the sounds in the two ears would grow less and less, and consequently the sound would be perceived to move downward along the secondary vertical circle. Then, if these two sounds were to act at the same time, the relation of the intensities of the sounds in the two ears would be like the relation of intensities with which a sound moving in the resultant direction would be heard by the two ears. Therefore the sound appeared in 1, 3, 5, 7 to move in the resultant direction. In 2, 4, 6, 8, 9, 10, 11 and 12 the relative difference increased gradually, for one sound was moved along the horizontal circle more towards the auditory axis, while the other sound was moved downward. Accordingly the sound appeared to travel more towards the side and at the same time more downward, i. e., along the direction resultant to those directions along which the two sounds were moved.

In the cases 13 to 26 one telephone was moved along the horizontal circle and the other was moved upward. In these cases it would be expected that the perceived sound would travel as before along the resultant direction. The results were quite perplexing, for though the sounds were sometimes perceived to travel along the resultant direction most of them were perceived, as shown in the Figure 17, to move downward along the direction nearly vertical to the resultant direction. In the figure *OI* and *OII* are the directions along which the two telephones were moved; *OA* is the direction nearly resultant to the above two directions; *OB* is the direction along which the perceived sound sometimes appeared to move.

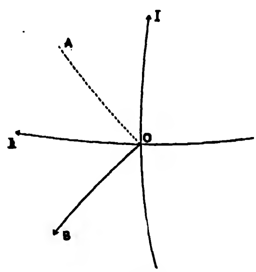


FIG. 17.

The explanation is not hard to find when we consider the fact that both the absolute intensity and the difference in the relative intensities become less and less when a sound is moved along the vertical circle upward. This decrease in intensity can be interpreted as the effect of the motion of the sound either upward or downward. If the former interpretation be taken the sound will be judged to move along the resultant direction, while if the latter be taken the sound will be judged to move along the direction nearly vertical to the resultant. Accordingly, the ob-

server may perceive the sound to move sometimes in one direction and sometimes in the other. But we must note here that the confusion is not restricted to the case in which the sound moving upward is taken for a sound moving downward, for sometimes the opposite happened as in 1 and 2, though not frequently. We must conclude that the upward and downward directions, under the condition of our experiments, are liable to be confounded with each other, owing to the similarity of the relation in the stimulation of the two ears.

The results of the experiments in this section tend to show that: (1) two component sounds of equal intensity at the same or different levels will give in combination a localization at the middle point between the two points at which the components are placed; (2) two component sounds of equal intensity which start at one and the same point and move simultaneously in different directions will give in combination a localization in the direction nearly resultant to the two directions along which the components are moved; (3) an occasional localization, under the above conditions, at the corresponding point in back instead of front, above instead of below and vice versa, arises from the confusion between front and back, above and below. All these localizations can be explained by the principle of relative and absolute intensities.

IV. Confusion between front and back.

When a source of sound is situated in the median plane the intensity of the sound heard by one ear is equal to that of the sound heard by the other ear, if the sensitiveness is the same for both ears; this is true whether the objective sound is situated to the front or to the rear. This is the cause of the confusion between front and back.

Ambiguity of the judgment as to whether a sound which is not in the median plane is to be localized in front or in back can be explained in a similar way. To one side of the observer and probably nearly in the auditory line there must be, as was noticed by Rayleigh,[1] one direction in which the ratio of the intensity of a sound as heard by one ear to the intensity of a sound as heard by the other ear has a maximum value which is greater than unity. For sounds situated in directions in front of this the ratio of the intensities has a less and less value, approaching unity as its limit when the sound is immediately in front. In like manner, for directions intermediate between the direction of maximum ratio and that immediately behind the observer, the ratio of intensities varies continuously between the same maximum value and unity. Accordingly, for

[1] Rayleigh, *Acoustical observations*, Phil. Mag., 1877 (5) III 456.

every direction in front there must be a corresponding direction behind for which the ratio of intensities has the same value; and these two directions are liable to be confounded with each other. The only directions as to which there is no ambiguity are the directions of maximum ratio itself, namely, right and left.

This view has been partly substantiated by the results of the foregoing experiments, but to make matter clearer I submitted it to the test of special experiments.

Two telephones were placed at *fr* 45° and *br* 45° on the same side of the observer at a distance of 60^{cm}. A short sound was to be given by one of them and the observer (Mr. K. Miura), a student of law, was to localize the sound. The intensity of the sound could be changed, as before, by means of the sliding inductorium. When the sound was of moderate intensity the observer could generally distinguish whether it came from the front or the back. But when the sound from the front became very weak he was liable to perceive it in the back, and projected it backward more towards the median plane when it grew weaker. Even when the sound from the front was of considerable strength he was sometimes liable to project it in the back. As for the sounds coming from the back, they were mostly projected in that direction.

A similar kind of experiment was made by using a watch instead of a telephone. The observer was seated, with his eyes closed, in the middle of a large room on a still evening. A watch was held at *fr* 45° or *br* 45° at a certain distance, and the observer was to tell the direction of the sound. In this case the result was the reverse of that for the telephone experiment. For here we found that the sound coming from the front was never localized in the back, while the sound coming from the back was frequently liable to be localized in the front, especially when it was more distant. In respect to the latter point there were some individual differences. One person projected almost all the ticks of the watch in front. Another person projected the ticks in the back when the watch was held at the distance of 50^{cm}, while he projected them in front quadrant when the watch was held at the distance of 100^{cm}. The general results of these experiments were, therefore, that when the sound coming from the back was situated near the ear, and was consequently stronger, the relative difference between the intensities of sensations in the two ears being also greater, it was generally localized in the back; whereas it tended to be localized in front when the sound was more distant and was consequently weaker, the relative difference between the intensities being also smaller. In the latter case the perceived sound tended to be localized more towards the median plane in front when the

objective point of sound was more distant and consequently the relative difference between the intensities of the sensations in the two ears was smaller, and it tended to be localized more towards the side when the objective point of sound was less distant and consequently the relative difference was greater.

RAYLEIGH conducted an experiment of similar kind by using two 256 v. d. forks and resonators, the observer being placed between them. At a given signal both forks were struck, but only one of them was held over its resonator. The observer was required to keep his head perfectly still, a very slight motion being sufficient in many cases to give the information that was previously wanting. In these experiments the observer facing north made mistakes between forks bearing approximately north-east and south-east, though he could distinguish without a moment's hesitation forks bearing east and west.

In connection with the above I may mention another kind of experiment which I conducted. From the fact that the perceived sounds were located in the median plane when two sounds were placed at symmetrical points on two sides, one on each side, I thought it possible that the same results would be obtained if two sounds were placed at diagonal points of the horizontal circle, for the relation of intensities of sounds received in the two ears might be equal in the latter case to the relation of the intensities of sounds received in the two ears in the former case. So I placed two telephones in the horizontal plane in six combinations such as: (1) *r* 22.5° and *l* 157.5°; (2) *r* 45° and *l* 135°; (3) *r* 67.5° and *l* 112.5°; (4) *r* 112.5° and *l* 67.5°; (5) *r* 135° and *l* 45°; (6) *r* 157.5° and *l* 22.5°. I found that under these conditions the localization of the perceived sound in the median plane was not so striking as was the case when the two sounds were placed at symmetrical points on the two sides of the median plane, one on each side, for though the observer located the perceived sounds at *b* or nearly at *b* for (2), (4) and (5) and at *f* or nearly at *f* and sometimes at *b* for (6), yet he located the sounds for (1) and (3) outside of the median plane. The results ran as follows: in case (1) the localizations were *b* or *for*, within the head (*bor*), within the head (*for*), within the head (*r*); in case (2) they were within the head (*b*), within the head (*b*), within the head (*b*), *b*, or *r*; in case (3) they were *b* or *r*, *fo*, *r*, *fr* 65°, *fr* 65°, *r*; in case (4) they were *b*, *b*, *b*, *b*, *b* (*u*); in case (5) they were *b*, *b*, within the head (*b*), *b* (*l*), within the head (*r*), *fl*, *fl*; in case (6) they were *f*, *f*, *f* or *b*, *f* (*l*), *f* (*l*), *f* (*l*).

On account of the comparative irregularity of these results I was doubtful whether the intensities of the sounds heard by the two ears under these

conditions were equal as I had at first thought. At any rate it was evident from these results that there was a possibility of finding two points in diagonal quadrants (i. e., quadrants at opposite ends of the same axis), one in each quadrant, which in combination will give a localization in the median plane. I learned afterwards that this possibility had been realized in the experiments conducted by MÜNSTERBERG and PIERCE[1] from which we can also conclude that the intensities of sounds heard in the two ears under the above conditions could not be regarded as exactly equal. From their experiments we learn that for any given point in either of the two quadrants upon one side of the median plane a point can be found in each of the two quadrants on the opposite side which in combination with the first will give a localization in the median plane at 0° or 180°. For example, a sound at *r* 45° will give 0° or 180° not only with its symmetrical *l* 45°, but also with a sound in the left back quadrant. Thus, for one of their observers B. *r* 45° gave 0° with *l* 105°, for another observer M. with *l* 115°, for W. with *l* 130°, for P. with *l* 140°, for N. *r* 45° gave 0° with *l* 45°, but 180° with *l* 130°, and for R. with *l* 125°.

MÜNSTERBERG and PIERCE regarded this as a special case of the more general principle: that for any given point in either of the two quadrants upon one side of the median plane a point can be found in each of the two quadrants on the opposite side which in combination with the first will give the same subjective localization. Thus their observer B. located *r* 10° + *l* 110° and *r* 10° + *l* 70° at *l* 20°; *r* 50° + *l* 10° and *r* 50° + *l* 130° at *r* 20°; *r* 100° + *l* 50° and *r* 100 + *l* 150° at 125°; *r* 120° + *l* 40° and *r* 120° + *l* 100° at *r* 40°. The results were similar with other combinations. Again, according to them, very similar to this principle is the fact that different individuals at different times locate a given combination in two different quadrants. Thus B. locates 0° + *l* 110° at *l* 60° and again at *l* 130°; *r* 30° + *l* 110° at *l* 40° and *l* 160°, etc. We may give the following as an illustration of the individual differences: sounds at 0° + *r* 135° by B. *r* 25°, by M. *r* 65°, by P. *r* 160°; sounds at 0° + *r* 160° by B. *r* 170°, by M. *r* 75°, by P. *r* 10°. The basis of these differences lies, they say, in the fact that not only 0° and 180°, but also other points before and behind, are confused when they are sounding in a combination. In the example 0° + *r* 135°, for instance, the judgment *r* 65° represents the middle; *r* 25° represents the middle, if *r* 135° is confused with the corresponding sound from the front at 45°; and *r* 160° represents the middle, if 0° is confused with 180°. Just so

[1] MÜNSTERBERG and PIERCE, *The localization of sound*, Psychol. Rev., 1894 I 461.

with $0° + r\,160°$, $r\,170°$ results if $0°$ stands for $180°$; and $r\,10°$ if r $20°$ stands for $r\,160°$.

All the foregoing results show that for a point given in the front quadrant a corresponding point can be found in the back quadrant on the same side of the median plane, which is liable to be confused with the first; and that the ambiguity or uncertainty of the judgment as to front and back is based upon the similarity which exists between the relation of stimulation of the ears by a sound in one quadrant and the relation of stimulation of the ears by the same sound in the other quadrant.

The discrimination between front and back seems to be based upon the absolute intensity, pitch and duration of the sound, to which the tactual sensations of the pinnæ and head may give some help, though the latter can not be made clear by experiment. The dependence of the discrimination upon the former was investigated by BLOCH,[1] as far as the discrimination between *f* and *b* is concerned. We can accept his results without further discussion, though they may not be applied to the discrimination between front and back in general. They are as follows.

BLOCH gave sounds at *f* and *b* respectively and made his observer judge from which of these two directions the sounds came. The results show that the correctness of judgment depends upon the pitch, intensity, duration and distance of the sound. When he used a tuning fork of 220 v. d. it seemed clear in general that a loud and long sound at the distance of 1^m was correctly judged as well in front as in back. A weak and short sound was not always localized correctly. A sound of greater intensity and duration—i. e., a sound of stronger acoustic excitation—made the perception of the direction in the median plane easier. When BLOCH made similar experiments with a pipe having a pitch of d_3 (1188 v. d.) the sound at a greater distance was localized better. When the distance of the sound increased the sound at the back appeared considerably weakened and the discrimination between front and back became easier. In the median plane a higher tone was localized better than a lower one. Again when the click of a snapper sounder was given at a distance of 2.4^m a weaker tone tended to be located more in back and a stronger tone more in front. With the increase of the intensity of tone the number of the *f* judgments increased and the number of the *b* judgments decreased, or with the decrease of the intensity of tone the number of the *f* judgments decreased and the number of the *b* judgments increased. We learn by experience, says BLOCH, that a certain sound is perceived with less intensity when it comes from the back than when it comes from the front. Accordingly when the direction of the sound is not

---

[1] BLOCH, *Das binaurale Hören*, Zt. für Ohrenheilk., 1893 XXIV 25.

clear we tend to locate a stronger sound in front and a weaker one in back.

In connection with the experiments on the confusion between front and back other experiments of somewhat similar kind may be mentioned. We have already seen that the value of the relative difference between the intensities with which a sound is heard by the two ears varies according to the direction of the sound. But the direction of the sound seems not to be the only condition upon which the change in the relative difference depends, for this difference seems also to depend upon the absolute intensity of the sound. In other words, this value seems to change, other things being equal, when the intensity of the sound changes. It has been a well-known fact since FECHNER'S [1] experiments that when two unison forks are held before the two ears respectively and one of them is more strongly sounded than the other, the single resulting sound appears to the subject to be heard entirely by the ear on the side of the stronger component. The ear which receives the weaker sound is said to become more or less "physiologically deaf." It seems to me that this "physiological deafness" of one ear becomes relatively greater when the sound received in the other ear grows stronger, and thereby the perceived sound tends to be projected much more towards the side on which the source of stronger sound is situated than when a sound of weaker intensity is used. The following experiments were designed to make this point clearer.

TABLE XXII.

Distance of the secondary coil for the back telephone.	Judgment of direction.	Judgment of distance.	
10^{cm}	*fr* 80°	8.5 ^{sun}	(25^{cm})
9	*fr* 80	9	(27)
8	*fr* 85	8.5	(25)
7	*r* 90	8.5	(25)
6	*r* 90	8.3	(25)
5	*br* 82.5	7	(21)
4	*br* 80	6.5	(19)
3	*br* 80	5.3	(16)
2	*br* 80	5	(15)
1	*br* 80	4.3	(13)
0.6	*br* 80	4	(12)

The number of experiments on each point is 4. The probable error for direction varies from 0 to $\pm 1 \frac{9}{10}$% and that for distance from 0 to $3 \frac{1}{61}$%.

[1] FECHNER, *Ueber einige Verhältnisse des binocularen Sehens*, Abhl. d. k.-sächs. Ges. d. Wiss., math.-phys. Cl., 1860 VII 339.

Two telephones were situated on the right side of the observer. One at 60° to the front and the other at 60° to the rear. The wires for the latter were connected with the secondary coil and the wires for the former with the primary coil of the sliding inductorium. In each experiment the intensity of the sound in front was kept constant, while that of the sound to the rear was varied. The subject of the experiments was C. W. Table XXII gives the results.

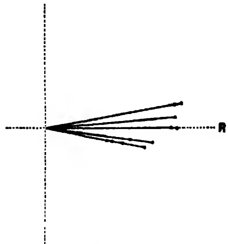


Fig. 18.

Figure 18 shows the results graphically. The perceived sound was gradually placed more towards the back as the sound to the rear grew stronger and consequently the relative difference became greater. The gradual change of the angular magnitude of the localized position of the perceived sound corresponding to the gradual change in the intensity of the sound to the rear is shown in Table XXII. An interesting point is that when the sound to the rear reached its maximum intensity—and consequently according to our view the relative difference between the intensities of the sensations in the two ears became greatest—the perceived sound was located at *br* 80°. Experimentally it seems to be the fact that when the relative difference is greatest the perceived sound is in general located somewhat to the rear of the visual right and left line.

Similar results were obtained when the experiment was conducted by placing telephones on the right side of the observer at 30° both in front and to the rear. Table XXIII gives the average results.

TABLE XXIII.

Distance of the secondary coil for the back telephone.	Judgment of direction.	Judgment of distance.
10^{cm}	*fr* 45°	10^{sun} (30^{cm})
9	*fr* 55	10 (30)
8	*fr* 57.5	9.5 (28)
7	*fr* 65	9.3 (28)
6	*fr* 67.5	9.5 (28)
5	*fr* 66.7, *br* 70 or *fr* 70	7.7 (23), 10 (30)
4	*br* 70, *fr* 75	7.1 (21), 7.5 (22)
3	*fr* 73, *br* 70	6 (18), 5 (15)
2	*br* 72.5	5 (15)
1	*br* 70	4.5 (13)

The number of experiments on each point is 4. The probable error for direction varies from 0 to $\pm 5\frac{7}{10}\%$ and that for distance from 0 to $\pm 10\%$.

Figure 19 shows the results graphically. As the sound heard by the right ear grew stronger the perceived sound was located more towards the side. But as the value of the relative difference between the intensities of the sensations perceived by the two ears was smaller in this case than in the last experiment, the perceived sound was never located so near to the auditory axis.

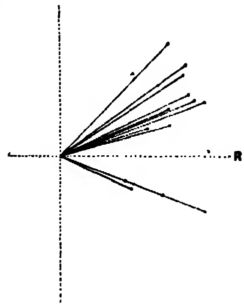


FIG. 19.

V. PERCEPTION OF DISTANCE.

The dependence of the perception of the distance of a sound upon its intensity has already been observed in foregoing experiments, though attention has been paid chiefly to the perception of the direction. In this section particular consideration will be given to the perception of distance with a view to determining under various conditions the relation between the intensity and distance of a perceived sound.

1. *Dependence of the change in the distance of a perceived sound upon the change in the intensity of the component sounds.*

Two telephones were situated on both sides at *r* 90° and *l* 90°. The wires from both telephones were connected with the secondary coil of the sliding inductorium. In this experiment it was requisite to make the intensities of the two component sounds equal in every respect at each distance of the secondary coil. This was done with a fair approximation to correctness. The subject of the experiment was C. W. In this subject the left ear was sharper than the right ear. Taking this fact into consideration we could not expect that the subject would locate all sounds

TABLE XXIV.

Distance of the secondary coil for both telephones.	Judgment of direction.	Judgment of distance.
9^{cm}	*f*	12^{sun} (36^{cm})
8	*f* (*l* 6.7°)	11.5 (35)
7	*f* (*l* 3.3)	11.3 (34)
6	*f* (*l* 5)	10.7 (32)
5	*f* (*l* 6)	9 (27)
4	*f*	6.7 (20)
3	*f*	8.7 (26)
2	*f*	7.7 (23)
1	*f*	6 (18)

The probable error for distance varies from 0 to $7\frac{4}{10}$%.

which he perceived strictly in the median plane. It would be more probable that when the intensities of sounds were weakened the observer would project the perceived sound a little towards the left of the median plane.

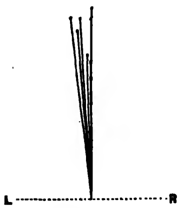


Fig. 20.

A current of $1\frac{1}{2}$ ampères was used in the primary circuit and the sounds were given in an arbitrary order. The average results of three experiments on each point were as given in Table XXIV. Figure 20 shows the results graphically.

When the same experiment was repeated with a current of 2 ampères the average results of three experiments on each point were as given in Table XXV.

Table XXV.

Distance of the secondary coil for both telephones.	Judgment of direction.	Judgment of distance.
9^{cm}	f (l 3.3°)	14^{sun} (42^{cm})
8	f (l 3.3)	12 (36)
7	f	10.7 (32)
6	f	9.3 (28)
5	f	9.3 (28)
4	f	7 (21)
3	f	6 (18)
2	f	4.3 (13)
1	f	4 (12)

The probable error for distance varies from 0 to $4\frac{7}{10}$%.

Figure 21 shows the results graphically. In both experiments the perceived sounds were located in the median plane, though when the sounds grew weaker the effect of the left sound became relatively stronger and the perceived sound tended to be localized a little towards the left of the exact median plane. The distance of the perceived sound gradually increased as the intensity of the component sounds grew gradually weaker.

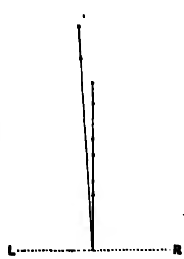


Fig. 21.

When the same experiments were repeated and the sounds were given in ascending or descending order the results were more regular, but not much different.

The results show that in the case of a familiar sound the judgment of its distance is based upon the difference in intensity.

A fact analogous to the results of these experiments is found in optics where the difference in distance is judged by the apparent magnitude of objects familiar to the sight and of known size.

2. *Relation between the perception of distance of a sound and its intensity.*

In the preceding experiments we have considered the change in the distance of the perceived sound which depends upon the change in the intensity of the telephone sounds owing to the change in the intensity of the electric current. By this method the quantitative relation between the distance of a perceived sound and the physical intensity of the sound cannot be found, for it is very difficult to measure the change in the intensity of a sound, either absolutely or relatively, as depending on the change in the intensity of the electric current. To obtain a quantitative relation I was, therefore, compelled to change the intensity of a sound by changing the distance of the sounding body. By that method the relation between the change in the perception of distance and the change in the intensity of a sound could be more easily established, for the intensity of sound-waves diminishes according to the law of inverse square of distance. I could not perform the experiment for a greater distance than 8 feet, for the cloth chamber within which I was compelled to execute the experiment to avoid reflection of the sound was a cube of 6 feet, the diagonal being about 8.5 feet long. The subject was seated in a chair in one corner of the chamber. A tape measure was stretched from that corner to the opposite corner on a level with the top of the head of the subject; the head was adjusted by a support in such a way that the tape measure would run in the median plane of the head. The point of the measure at which it was intersected by a perpendicular drawn from the middle point of the imaginary line connecting the openings of the ears was regarded as the zero point. I used a telephone sound which was connected with an electro-magnetic fork of 250 vibrations per second. Between the fork and the telephone a short circuit key was inserted, by means of which the duration of the telephone sound could be regulated. The telephone was to be moved below the tape measure and parallel to it, so that it was situated in the median plane at the level of the openings of the ears.

The first step of the experiment was to find the point at which the subject judged the sound to be distant just one foot. Let this point be called *A*. The next step was to give two short sounds with a brief interval between them, the first at the point *A* and the second at a different point. The subject was to judge, with his eyes closed, whether the second sound was twice as far distant as the first sound. If the subject thought that the second sound was nearer or farther than twice the distance of the first sound then the experiment was to be repeated by giving the second sound at a farther or a nearer point. After many experiments a point would be found at which the second sound appeared to be just twice

as far as the first one. In a similar way the points were found, at which the second sound was judged to be three times, four times and five times as distant as the first sounds. In this way the relation between the distances in the mental and physical scales was established. During the experiment the intensity of the telephone sound was kept constant, so that the change in the intensity of the perceived sound would arise only from the change in the distance of the sound, and could be expressed by the accepted law of the propagation of sound.

I made the experiments on the two subjects I. M. and K. M. More than 100 experiments were tried for each point of the scale of distance, care being taken to avoid the effect both of practice and fatigue.

Both subjects judged the sound to be about one foot (3.1^{cm}) distant when the sound was given at the point 40^{cm} distant from the 0 point of the tape measure. For the subject I. M. the relative mental scale of the distances 1, 2, 3, 4, 5 corresponded to the physical scale of the distances 40, 80.9, 112.4, 159.3, 185.9 cm., and for the subject K. M. the relative mental scale of the distances 1, 2, 3, 4, 5 corresponded to the physical scale of the distances 40, 79.2, 117, 152, 193.5 cm. Now as these physical distances are the distances between the middle point of the auditory axis (connecting the ears) and the telephone, and as the distance between this point and the opening of the ear is about 8^{cm}, we have for the square of the distance between the ear and the telephone the sum of the squares of the above two distances, i. e., the distance between the telephone and the middle point of the auditory axis and the distance between the middle point and the opening of the ear, for these three distances correspond to the three sides as a right-angled triangle. Finding the squares of the distances between the ear and the telephone, which correspond to the distances in the mental scale, we have for I. M., 1664, 6609, 1298, 25440, 34623. Dividing these figures by 1664 (to get the relative distances) we have the ratios 1, 3.3, 7.5, 15.4, 20.8. The squares of the distances for K. M. are 1664, 6337, 13753, 23168, 37506. Finding the ratios we have 1, 3.8, 8.3, 13.9, 22.3.

The ratios in the above two cases are nearly equal to the squares of the distances of the sounds in the mental scale, namely, 1, 2, 3, 4, 5. As the reciprocals of these ratios represent the relative intensities of the sounds, the conclusion appears justified that when the intensity of a sound diminishes in geometrical progression the perceived distance of the sound increases in arithmetical progression.

3. *Continual change in the distance of a perceived sound due to the movement of the two component sounds.*

In connection with the question of distance I conducted other sets of

experiments in which the objective positions of the two component sounds were simultaneously changed, while their intensities were kept constant, and obtained results which showed again the dependence of the perception of distance upon the change in the intensity of the perceived sound. In these experiments the spherical cage (Figure 1) was used.

a. The two telephones were moved along the radii of the horizontal circle of the cage. The directions in which the two telephones were moved are as given below. The arrows indicate the directions, and the letters indicate the points of the spherical cage as explained on p. 3; thus the expression for Number 1 means that the two telephones were placed at first very near to the ears and they were then moved simultaneously to the points *r* and *l*; Number 3 means that they were started at the front and the back very near to the head and were moved to *f* and *b*; Number 5 means that one of the two telephones was started at *r* and moved to the ear, while the other telephone was started at the left ear and moved to *l*; etc.

1. $l \leftarrow$ ear ear $\rightarrow r$; 2. $l \rightarrow$ ear ear $\leftarrow r$;
3. $f \leftarrow$ head $\rightarrow b$; 4. $f \rightarrow$ head $\leftarrow b$;
5. $l \leftarrow$ ear ear $\leftarrow r$; 6. $l \rightarrow$ ear ear $\rightarrow r$.

Five or eight experiments were made for each case and the judgments of the observer T. N. in regard to the directions of the perceived sounds under the conditions were as given below; *n* means the nose, *x* means doubtful, *k* means within the head.

1. *k–f, k–x, k–b(x), k–b(x), k, k(f), k–f and* $l \leftarrow k \rightarrow r$, *k–f and* $l \leftarrow k \rightarrow r$;
2. *f, b–k, b–k, b–k, b–k, b–k, f–n, k(f)*;
3. *k–f, n–f, n–f, k(f)–f, n–f*;
4. *f–n, f–n, f–n, f–n, f–n, f–n*;
5. *l–r, l–r, l–r, l–r*;
6. *r–l, r–l, r–l, r–l, r–l*.

In Number 1 the two ears of the observer were stimulated at first by strong sounds and he felt the sound in the interior of his head. As the telephones were moved along the auditory axis farther and farther from the ears the intensities of two sounds grew weaker and the perceived sound emerged from the head and receded along the median plane more and more towards *f*. But it receded occasionally towards *b*. This result shows that the continual change in the distance of the preceived sound arises from the continual change in the intensity of the sound. In this experiment the observer perceived sometimes two sounds separately, though at the same time he perceived a third sound in in the median plane. Number 2 is just the reverse of Number 1. The sound was

perceived at first in front or back at a certain distance and then it gradually approached towards the head and at last entered the head.

It is interesting to note here that in Number 2 the sound seemed to start generally at *b* while in Number 1 it seemed to stop generally at *f*, though the intensity of the stimulation of the ears at the instant of starting in Number 2 was just like the intensity of the stimulation of the ears at the instant of stopping in Number 1. In Number 1 the observer was sure that the sound was in front, for in the first half of the experiment the perceived sound was very strong, and therefore he continued to think that the sound was still moving in the same direction even when its intensity grew weaker and weaker. He thought only occasionally that the sound moved towards the back, but he was very doubtful of his judgment. In Number 2 the case is a little different. In this case the sound was heard with less intensity at the first moment than in the succeeding moments, and the observer was in doubt at first whether it was in front or in back. It was his usual experience that when a sound was at the back he was generally in a state of doubt. So in this case he judged that the sound started from the back, and consequently he continued to think that the sonnd was still moving from back even when the sound grew stronger. Thus 1 and 2 show that the perception at a certain instant is influenced by the perception of the preceding instant.

In Number 3 the sound in front was heard with greater intensity than the sound at the back at the same distance. Moreover the sound was heard at first with great intensity. The observer could not, therefore, doubt that the sound was in the front very near to the head. As the intensity became less the distance of the perceived sound increased. Number 4 is the reverse of Number 3 and needs no explanation.

Most interesting results were given by Number 5 and Number 6, in which we found striking examples of the dependence of the perception of distance upon intensity. In these cases the two telephones were moved in the same direction and not in the opposite direction as in the previous four cases. Here the observer perceived the sound to be travelling in a direction reverse to the direction of the movement of the telephones. In Number 5 one telephone was moved from *r* to the right ear, while the other telephone moved from the left ear to *l*, so that the direction of movement of the two telephones was from right to left. The observer perceived the sound to be travelling from left to right. The explanation is simple and clear. At first the sensation of the left ear was relatively stronger, and gradually grew weaker, whereas the sensation of the right ear was at first relatively weaker and gradually grew stronger. At a certain point the intensities of the two sensations became equal. Con-

sequently the sound was at first perceived on the left side near to the ear, then it was perceived in the median plane, and at last it was perceived on the right side near to the ear. The intermediate points were passed in succession. As a whole the sound seemed to have travelled from left to right. Number 6 is just the reverse of Number 5.

All the six cases show that the distance of the perceived sound depends upon the intensity of the sound. The change in the former is continuous when the change in the latter is continuous. Continuous change in the intensity of a sound is always perceived as a change in the distance of the sound, i. e., as a motion of the sound, whether that change in intensity is caused by actual motion or not.

b. The two telephones were moved along the circumference of the horizontal or frontal circle of the spherical cage. The objective path along which the two telephones were moved were as indicated below. The expression for Number 1 means that the two telephones were started at the front and were moved around simultaneously to the left and right; Number 3 means that they were started at the left and right and were moved simultaneously toward the front, etc.

1, $l \leftarrow f \rightarrow r$; 2, $l \leftarrow b \rightarrow r$; 3, $l \rightarrow f \leftarrow r$; 4, $l \rightarrow b \leftarrow r$; 5, $l \leftarrow o \rightarrow r$; 6, $l \leftarrow u \rightarrow r$; 7, $l \rightarrow o \leftarrow r$; 8, $l \rightarrow u \leftarrow r$.

The number of experiments for each case was from 4 to 6. The judgments of the observer T. N. in regard to the directions of the perceived sounds under these conditions were as follows:

1. *f–n, f–n, f–n, f, f–fo–o.*
2. *b–k, b–k, b–f, b–bo– o* or *f–n.*
3. *k–f, f, k*(*b*) or *k*(*f*)*–f, b–x–f.*
4. *k–b, k–b, k–b, n–b.*
5. *o–k, o–k, o* or *f, o–b.*
6. *u–n, u–k, u–uf–f, u–uf–k.*
7. *n–f, f–b, f–q, f–o, k–o*(*b*)*, f–fo–o.*
8. *n–u, n–u, n–u, k–u.*

In all cases the intensities of the sounds for the two ears were equal and the perceived sound was always located somewhere in the median plane. Special results were observed as follows.

Number 1. When the telephones were moved from *f* to *r* and *l*, the sound was perceived to travel in the median plane from some distant point in front inward to the nose or the head of the observer. This decrease of distance presumably arose from the increase of the intensity of the perceived sound, for under these conditions the perceived sound is stronger on account of the action of the pinnæ, when the objective sounds are situated more towards the auditory axis than when they are situated

more towards front. But as the tragus has some influence upon the intensity of the perceived sound, the observer was sometimes in doubt about the direction of motion of the sound as the telephones were passing about the points *fl* and *fr*.

Number 2. When the telephones were moved from *b* to *r* and *l* the sound was perceived to be travelling from *b* to *k* or from *b* to *f*. This needs no explanation, for it is analogous to Number 1.

Number 3 and Number 4 are just the reverse of Number 1 and Number 2 and the sounds were perceived to pass from *k* to *f* and from *k* to *b* respectively.

Number 5 and Number 6 can be explained in a similar way. On account of the pinnæ and the direction of the external meatus, sounds coming from above or below are heard with less intensity than sounds coming from the direction of auditory axis. Accordingly the intensity of the perceived sound increases gradually as the telephones are moved from above or below toward the auditory axis, and the distance of the perceived sound seems to decrease.

Number 7 and Number 8 are the reverse of Number 5 and Number 6.

It is interesting to note that in Number 7 the sound was sometimes perceived to have travelled from *f* to *b*, or *f* to *o*, or *n* to *f*, instead of travelling from *k* to *o*. From this we can see that in discriminating directions in the median plane much depends upon the interpretation of the observer. In number 7 the stimulation of the ears was strongest in the beginning; then grew less and less as the telephones were moved gradually upward. This decrease of the intensity can be interpreted by the observer as the effect of motion of sound from *f* to *b*, or *f* to *o*, or *n* to *f*.

Finally in connection with the question of distance we must call attention to the endocephalic localization which we have already noticed. When the intensity of the two sounds opposite the ears becomes very great the perceived sound, which is localized at first in the median plane, approaches the head and at last enters it. This endocephalic localization is sometimes so strong that the subject cannot get rid of the illusion, though he knows perfectly well that the objective sounds are outside the head. This illusion occurs more strikingly when the telephones are placed against the ears or when conducting tubes are put into the ears. Under such conditions the sound is heard in the head even if the intensity is not very great. An experiment of SCHAEFER[1] is interesting in this connection. In his experiments a telephone was

[1] SCHAEFER, *Zur interaurealen Localisation diotischer Wahrnehmungen*, Zt. f. Psychol. u. Physiol. d. Sinn., 1890 I 300.

brought near to a funnel receiver, which communicated with the two ears by means of a forked tube with arms of equal length. The telephone was connected with the secondary coil of a sliding inductorium. At the start the secondary coil was put at a great distance from the primary coil of the inductorium and then the secondary coil was gradually brought nearer to the primary, during which the change in localization was observed. It was found that the apparent sound approached the head according to the decrease of the distance between the primary and secondary coils, so that the sound finally crept into the head and occupied a position between the ears. If one of the arms was closed the sound would shift to the auditory canal of the opposite ear. If the secondary coil were then moved farther from the primary coil the sound would go out from the auditory canal to the space on the side of that ear. If the pressed tube were then opened, the sound would move to the median plane at some distance from the head. With many persons the sound entered or emerged from the head at the root of the nose according to the increase or decrease of the intensity of the telephone sound.

VI. The least perceptible change in the direction of a sound.

In our previous experiments we found that when two telephones were placed on opposite sides just in the line of two ears and the intensity of one component of the perceived sound was changed while that of the other was kept constant, the perceived sound was located more toward the side on which the objective sound was stronger, and more toward the median plane when the intensities of the two component sounds became more nearly equal. While conducting these experiments I noticed occasionally that, when the relative intensities of the two sounds were in such a relation that the perceived sound was located at *r* 90° or *l* 90°, a small change (1 to 1½ cm. for the secondary coil) in the difference between the relative intensities of two sounds was not usually perceived as a change in the direction of the perceived sound and, in fact, was not perceived at all. But when the intensities of the two component sounds were in such a relation that the perceived sound was located just in front, the change in the difference between the relative intensities corresponding to 0.5^{cm} or 1^{cm} was usually perceived as a change in direction.

The explanation of the difference in the above two cases seems to me to lie in the fundamental fact of sensation as expressed by Weber's law. For the initial difference between the relative intensities of the sounds heard by the two ears was greater in our experiments when the perceived

sound was located more toward the side, and it was smaller when the perceived sound was located more toward front. Consequently a large change would be necessary in the former case if the change is to be perceived.

The object of the next experiment was to make this relation clear. As in my previous experiments, two telephones were placed on opposite sides. The distance between the ear and telephone was 45 cm. The intensity of one component sound was kept constant, while that of the other component sound was varied by means of the secondary coil; the observer localized the perceived sound at a certain point as before when the two components were given simultaneously. This point was to be regarded as the initial direction. Then, while the two sounds were given, one of them was to be moved from its original position slowly along the auditory axis till the point was reached where the observer just noticed a change in the initial direction of the perceived sound. The distance traversed by this component sound was measured by a millimeter scale which was fixed along the path of the telephone. This distance is called an increment distance (Δ), for it is a distance which is required for producing the least change in the initial direction of the perceived sound.

The results of the experiments on the relation between the change in the initial direction and the corresponding increment distance were as given in Table XXVI.

TABLE XXVI.

The right component sound constant. Observer: C. W.

M	*D*	*I*	Δ	*E*	*n*
27	0.6 cm	*f*	13.1 cm.	*r*	15
	2	*fr* 30°	15.6	*r*	15
	3	*fr* 70	21.7	*r*	15
	4	*r* 90	26.1	*r*(*b*)	15
	5	*r* 90 (*b*)	27.5	*r*(*b*)	7

M, date in March, 1897.

D, distance of the secondary coil for the left telephone.

I, direction of the perceived sound for the position *D*.

Δ, change in the distance of the left telephone.

E, direction toward which the sound appeared to change.

n, number of experiments.

The probable error of Δ varies from $\pm \frac{8}{10}\%$ to $\pm 1\frac{7}{10}\%$.

In this experiment the intensity of the sound produced by the right telephone (connected with the primary coil of the inductorium) was kept constant while the intensity of the sound produced by the left telephone

(connected with the secondary coil) was varied. When the secondary coil was moved away from the primary to the point 0.6^{cm} and sounds were given from both telephones at the same time the observer located the single resultant sound at *f*. The resultant sound was localized at *fr* 30°, *fr* 70°, *r* 90°, *r* 90° (*b*) respectively as the coils were separated as 2^{cm}, 3^{cm}, 4^{cm}, 5^{cm}.

When the observer located the sound at one of above directions the intensity of the left component was decreased again by moving, not the secondary coil, but the left telephone itself from the ear along the auditory axis, till the point was reached at which the sound was just noticed to shift from its initial position.

As we find in the table the increment distance was least when the perceived sound was initially located at *f*, it increased gradually till it reached its maximum when the perceived sound was initially located at *r* 90°(*b*). When the initial difference between the intensities of components heard by the two ears was much greater the increment distance became greater than 35^{cm}. In our small room we could not make an arrangement to move the telephone farther than 35^{cm}. The reason why the perceived sound shifted its position always toward the right in this experiment is easy to find, for the intensity of the right component became relatively greater as the intensity of the left component decreased. We must notice here the fact that the perceived sound was located at *r* 90°, but a little backward than just at *r* 90°, when the relative difference between the intensities of sounds in the two ears was very great. A similar case has been already observed in our previous experiments. I have said that it is probable that what we call commonly right and left lies in the line drawn tangent to the front surfaces of the two eyeballs. So when the maximum difference in the intensities of the components in the two ears is obtained, the perceived sound is not located just at visual right or left, but, being referred to the common standard of direction, it is located at right or left, a little toward back. Or we may say that the difference in the relative intensities with which a sound is heard by the two ears is greatest, not when the sound is situated in the visual *rl* line, but when it is situated a little behind it. On this basis the fact that the sound seemed to shift a trifle backwards from *r* 90° when the intensity of the right component became relatively very strong does not disprove the dependence of the least perceptible change in the direction of a sound upon the change in the relative difference between the intensities of the components in the two ears. From similar experiments conducted on a different day, results were obtained in which the above relation can be seen still more clearly (Table XXVII).

Owing to the slight changes in the position of the telephones and the head of the observer and minute changes in the intensity of the sound arising from the fluctuaton of the electric current, there were some deviations in conditions for different days; and consequently the results of the experiments must be considered for each day separately.

TABLE XXVII.

The right component sound constant. Observer: C. W.

M	*D*	*I*	*Δ*	*E*	*n*
24	1 cm	*l* 90°	24.9 cm	*f*	15
	2	*bl* 70	17.9	*b*	15
	4	*f*	12.5	*r*	15
	4.5	*b*	15.7	*r*	15
	6	*fr* 60	14.2	*r*	15
	7	*r* 90	25.7	*r*(*b*)	15

Notation same as in Table XXVI.

The probable error of Δ varies from $\pm 1\frac{4}{10}\%$ to $\pm 2\%$.

Some experiments made upon Dr. Scripture gave similar results which are given in Table XXVIII.

TABLE XXVIII.

The right component sound constant. Observer, Dr. Scripture.

M	*D*	*I*	*Δ*	*E*	*n*
27	0.6 cm.	*fl* 10°	23.3 cm.	*f*	9
	3	*f*	20.7	*r*	10
	5	*fr* 60	22.7	*r*	10

Notation same as in Table XXVI.

The probable error of Δ varies from $\pm 2\frac{5}{10}\%$ to $\pm 3\frac{5}{10}\%$.

In these experiments the perceived sounds were sometimes located to the rear. In such cases the least change in direction was also made toward *b*. Again, when the initial direction was on the left side the least perceptible change was made toward the median plane. The shifting of of the left sound toward the median plane is really a shifting toward the right side, and can be explained by the relative increase of the intensity of the right component sound.

Similar experiments were made upon C. W. on different days, by varying the intensity of the right component sound while keeping that of the left component sound constant, and similar results were obtained, which are given in Table XXIX and need no special explanation.

The left component sound constant. Observer: C. W.

M	*D*	*I*	*Δ*	*E*	*n*
18	3^{cm}	*r* 90	29.1^{cm}	*l*	14
	5	*fr* 70	26.4	*l*	18
	8.25	*f*	15.1	*l*	15
19	10	*l* 90	24.6	*l*(*b*)	20
	7	*fl* 70	22.4	*l*	20
	6	*f*	20.5	*l*	19
20	2	*br* 80	34.7	*b*	5
	4	*fr* 20	24.4	*f*	12
	7	*l* 90	33.2	*l*(*b*)	4
31	11	*fl* 80	33.6	*l*(*b*)	15
	10	*fl* 20	21.4	*l*	17
	9	*f*	17.6	*l*	15
	7	*fr* 10	25.6	*l*	13
	5	*fr* 20	31.7	*l*	15

D, distance of the secondary coil for the right telephone.

Other notations same as in Table XXVI. The probable error of *Δ* varies from $\pm \frac{4}{10}\%$ to $\pm 2\frac{5}{10}\%$.

As the intensity of sound is inversely proportional to the square of distance, the intensity of the variable component sound in the foregoing experiments is proportional, other things being equal, to $\frac{1}{(C+\Delta)^2}$ where C represents the initial distance (45^{cm}) between the telephone and the ear and Δ represents the increment distance. As C is constant in our experiments, the value of the fraction depends upon Δ. The greater the change in the increment distance, the greater is the change in the intensity of the component. But the change in the increment distance which is required for producing the least noticeable change in the initial direction would be expected according to WEBER's law to depend upon the value of the initial difference between the intensities of the two components. This expectation was realized in the actual results of the foregoing experiments. For in these experiments the increment distance was smallest when the sound was initially located in front or behind, that is, when the initial difference between the intensities of the component sounds in the two ears had the smallest value possible (namely, zero). The increment distance increased gradually as the initial difference between the intensities of the component sounds in the two ears became greater, and consequently as the perceived sound was initially located more towards the right or left side.

VII. Theoretical conclusions.

Sounds are located both as to direction and distance from ourselves as the center. In the foregoing experiments we have found that the judgment

of the direction of a sound depends chiefly on the relative difference between the intensities of the component sounds heard by the two ears; while the absolute intensity of the perceived sound is supplementary to this fundamental factor. The judgment of the distance of a sound depends, on the contrary, chiefly on the absolute intensity. To these factors in the localization of a sound we may add other coöperating factors, such as relative and absolute pitch, timber and phase.[1] With these data at hand it becomes necessary to inquire how they bring about the localization of a sound. There are several theories of the way in which this is done; these will be briefly discussed.

1. *Theory of a direct acoustic space.*

This theory assumes that a sound heard by the right ear is distinguishable from that heard by the left ear; that the right and left components of a sound heard with both ears produce an effect in consciousness which varies with the relative intensity of the components; that this effect is an experience analogous to that from simultaneous but not identical sensations of sight or touch; that it should be considered to be one of the kinds of space analogous to visual space or tactual space; and that this acoustic space is brought into relation to and modified by visual, tactual and muscular experiences.

This might be called a *direct* theory of acoustic space. Although no positive objection can be made, it seems somewhat artificial and not well adapted to explain the methods by which we localize sounds.

2. *Tactual theory.*

Some psychologists have sought the ultimate origin of acoustic space in the tactual sensations of the tympanic membrane.

One form of this theory assumes: 1, that special sensations of touch are received from different parts of the tympanum; 2, that the sound-wave arouses the sensations from the tympanum; and 3, that different

[1] Urbantschitsch, *Zur Lehre von der Schallempfindung*, Archiv f. d. ges. Physiol. (Pflüger), 1881 XXIV 574.

Rayleigh, *Our perception of the direction of a source of sound*, Nature, 1876 XIV 32. Rayleigh, *Acoustical observations*, Phil. Mag., 1877 (5) III 456.

Mach, *Bemerkung über die Function der Ohrmuschel*, Arch. f. Ohrenheilk., 1875 n. F. III 72.

Thompson, *On binaural audition*, Phil. Mag., 1877 (5) IV 274; 1878 VI 383; 1881 XII 351. Thompson, *On the function of the two ears in the perception of space*, Phil. Mag., 1882 (5) XIII 406.

Bloch, *Das binaurale Hören*, Zt. f. Ohrenheilk., 1893 XXIV 25.

parts of the tympanum are affected according as the direction of the sound is different.[1]

The first assumption is in all probability justified; the tympanum is undoubtedly sensitive and the stimuli applied to different portions can be presumably distinguished.

The second assumption is not so readily to be accepted in view of the fact that the energy of the air-vibrations is extremely small, in fact so small that a most complicated apparatus, the internal ear, has been specially adapted to transform the air-vibrations into nerve-currents. There is no conclusive experimental evidence to support the view that sound-waves can be directly felt by the end organs or the nerves of touch. In the perception of sounds the tympanum is not the true receptive organ, but only a part of a mechanical device by which the vibrations are gathered and conveyed to the internal ear.[2]

The third assumption is quite untenable. The diameter of the auditory canal is too small to permit of different degrees of pressure at different points of the tympanum at the same moment; we can without hesitation consider the pressure as equal over all parts of the tympanum.[3]

A second form of this theory to the effect that the touch organs of the tympanic membranes in the two ears are affected differently by sounds coming from different directions would be consistent with observations of cases in which the ability to localize sounds was lost, together with the sensitiveness of the tympanum.[4] In spite of some minor observations that may be interpreted in favor of this theory it is difficult to agree with WUNDT[5] in accepting it even partially; at any rate such an extremely remarkable sensitiveness of the tympanum should be most thoroughly established before a final decision could be given in favor of it.

3. *Theory of a special space organ.*

According to PREYER the semicircular canals are the organs for the perception of the direction of a sound.[6] By means of a specific energy of the

---

[1] KUPPER, *Ueber die Bedeutung der Ohrmuschel des Menschen*, Archiv f. Ohrenheilk., 1874 n. F. II (3) 158.

[2] THOMPSON, *On the function of two ears in the perception of space*, Phil. Mag., 1882 (5) XIII 406.

[3] HELMHOLTZ, *Die Mechanik der Gehörknöchelchen und des Trommelfells*, Archiv. f. d. ges. Physiol. (Pflüger), 1868 I 1.

[4] GELLÉ, *Rôle de la sensibilité du tympan dans l'orientation au bruit*, Soc. de Biol., 1886 III 448.

[5] WUNDT, Physiol. Psychol., II 94, Leipzig, 1893.

[6] PREYER, *Die Wahrnehmung der Schallrichtung mittelst der Bogengänge*, Archiv. f. d. ges. Physiol. (Pflüger), 1887 XL 586.

ampullæ of the semicircular canals a particular kind of sensation is produced when they are excited by a sound from a particular direction. The sensation is different according to the direction from which the sound comes, because a sound must stimulate more strongly one canal or pair of canals in a way depending on its direction. As the six canals are excited with different relations of intensities by sounds from different directions, the sensations which are produced by the specific energy of the ampullæ are different corresponding to the different directions of the sounds. These differences of sensations give us the ideas of the directions of sounds. "If we consider," says PREYER, "that by the stimulation of an ampulla with the vibration of the liquid of the canal belonging to that ampulla a sensation of sound is produced which is different from the sensation produced by the stimulation of another ampulla, though the two are exactly alike in respect to intensity, pitch and timbre, and if the difference between the two arises simply because the different nerve-fibers are stimulated, we cannot but regard this difference as spatial."

PREYER's assumption that a canal is most strongly stimulated by a sound lying in the plane in which the canal lies is irreconcilable with the elementary facts of the physics of sound. The sound-waves which are transmitted through the air reach the membrana tympani and then move the chain of ossicles, and at last the lymphatic liquid of the inner ear. During this conduction the sound-wave is changed into a movement of a membrane, a movement of a set of bones, a movement of a liquid, etc. There is no possibility of any change of direction of these movements for changes in the direction of the sounding body, and consequently no possibility of a varied action on the semicircular canals.

PREYER may have thought that the sound-waves are conducted directly from the air through the skull bones into the head and thus to the semicircular canals; at least, this seems the only possible construction of his view. The conduction of sound-waves by the cranial bones comes into play when a sonorous solid body is either in immediate contact with the skull, or is connected with it by a chain of solid or fluid bodies, or when the medium immediately surrounding the head is not gaseous as, for instance, when the head is immersed in water. With aquatic vertebrates we can not deny the cranial conduction of sounds, but for most sounds it is certain that in the case of human beings the cranial conduction plays no part. The sound waves are mostly reflected from the surface of the head, and the conduction directly through the cranial bones and tissues is infinitesimal.

PREYER's theory next involves the assumption that the sound-waves

affect the canals differently, according to the direction of the sound. He evidently viewed the sound as a force entering the head and being resolved into three components with intensities depending upon the angles made by the direction of the force with the planes; he had probably in mind the familiar method of resolving a movement or a velocity into three components in planes at right angles to each other. The absurdity of such a position is at once evident to any one acquainted with the elementary ideas of wave-motion. A sound-wave passing through the air consists of alternate condensations and rarefactions—not of a line or of a plane—but of a more or less spherical surface. The sound-wave passing through the head is of a similar sort. There is not the remotest reason for believing that the molecules of a liquid in a curved tubular bone will vibrate differently according as the sound-wave approaches from different directions in the mass in which the bone is imbedded.

This theory of PREYER'S was accepted by MÜNSTERBERG[1] as the starting point of a reflex-muscular theory of acoustic space. "The different movements of the head, which can be aroused by stimulation of the semicircular canals, arouses—by means of the muscle sense—that threefold system of sensations of movement which forms the basis of our acoustic space." "To localize a sound means to assign to its place in the whole system of sensations of head-movements the sensation of that reflex head-movement which is necessary in order to turn toward the source of the sound."

Concerning the possibility of the direct stimulation of the semicircular canals nothing further needs be said.

As for the results of MÜNSTERBERG'S experiments on the least perceptible change in the direction of a sound, they cannot be regarded as a proof of his theory. It has been made clear by the experiments of BLOCH[2] and by mine that the curve of the least perceptible change can be explained by the principles of relative and absolute intensities.

4. *Motor theory.*

The essential factor in the motor theory seems to be this: A sound perceived by the ear brings with it an impulse—conscious or unconscious—to move toward it; this motor impulse and its results are what appear to us as the localization of the sound. This theory assumes that these motor impulses are aroused in definite relations by the factors men-

[1] MÜNSTERBERG, *Raumsinn des Ohres*, Beiträge z. exper. Psychol., 1889 II 182.

[2] BLOCH, *Das binaurale Hören*, Zt. f. Ohrenheilk., 1893 XXIV 25.

tioned at the beginning of this section (p. 70), namely, absolute intensity of the sound, relative intensity for the two ears, etc. The form of the motor space is derived from past experience under influence of the visual space; in fact, the space in which the sound is localized is our usual visual-tactual-motor space with which the sound is connected by the motor impulses. In this compound space the visual sensation is the most important element. The tactual sensation seems rather to be of secondary importance except in the case of the blind. Speaking genetically we can recognize the position of a sound only when acoustic and visual sensations are connected with each other in a definite relation. It is unthinkable that we can recognize the position of a sound without connecting the former directly or indirectly with the latter. A definite connection between these two can be accomplished through the medium of a definite muscular action. These three elements have occurred in connection with each other, and have been firmly associated in the course of time, so that when one of them is present the others will be necessarily called forth. When the visual sensation fails, as in the case of a blind person, then tactual sensation takes its place.

From a biological point of view this theory seems quite natural. In the course of natural selection the survivors have obtained the power of reacting suitably to the different acoustic sensations which they receive from the surrounding world.

To react suitably upon an acoustic sensation we must first of all recognize the direction from which the sound comes. To recognize the direction it is necessary that the auditory sense should be assisted by other senses. But as the auditory sense has to do with more or less distant object, it must be chiefly the visual sensation which is combined with the auditory sensation to assist it in perceiving the direction of the sonorous object. To perceive an object in a certain direction the head must be moved so that the object will be brought in the line of visual fixation or in the median plane. In the beginning this will not be easily done, but after practice it will be found that the instant at which the sound is equally loud in both ears is the instant at which the source of sound is found in the median plane. Again we have already seen that a sound situated in the median plane will be best heard when it is in the line of sight. Thus after the source of sound has been brought into the median plane, the next process will be to bring it into the line of sight.

Again we must notice the fact that by practice it has been found that the instant at which the sound is perceived with greatest intensity by one ear and with smallest intensity by the other ear is the instant at which the source of sound is situated nearly in the auditory axis or little

behind it. Accordingly, we sometimes try, by the movement of the head, to bring a source of sound into this axis when we find it more convenient, as for a sound of small intensity.

These movements of the head, which we perform in order to connect visual and acoustic sensations, affect our sense of equilibrium, by means of which we become conscious how much we have turned our head. By means of this sense of equilibrium we are enabled to estimate the position of a source of sound in reference to the position of our body.

Again, though a source of sound is usually brought into the line of fixation of sight by the motion of the head, we can effect this within a certain limit not by moving the head but by moving the eyeballs alone. In such a case the acoustic sensation is connected with the oculo-motor sensation, by means of which we can feel also the angular direction of a source of sound, for we have a fine sense for this movement of the eyes. This muscular sensation of the eyes is supplementary to the sensation of rotation of the head, by means of which we can chiefly estimate the direction of a sound.

From these considerations it is clear that by long practice the association has been established between a particular acoustic sensation—corresponding to the stimulation by the sound from a particular direction—and the rotatory sensation—which is required for bringing the source of sound into the visual fixation line or into the auditory axis—and moreover the association has been established between the acoustic sensation and the oculo-motor sensation. After the associations between these factors have been established, either one of these factors by itself is able to call forth other factors. It is not necessary that the rotatory sensation or muscular sensation, which gives us the measure of the angular departure of a source of sound from the fixation line of the eyes, should arise always by an actual movement. Though this is the original case, the sensation of innervation or reproduced motor sensation called forth by association with the acoustic sensation may take the place of the actual motor sensation.

Our final conclusion is thus that an acoustic sensation receives its spatial form primarily from the space idea which is given to us by the visual, tactual and motor sensations. Acoustic space presupposes the existence of the space form of other sensations. We have only to give an account of how the perception of the position of sounds arises on the basis of the already existing space which was given to us by other sensations. As to the further problem of the ultimate origin of the space form of perception, its solution must be sought in the visual and tactual perception.

ON BINAURAL SPACE.

BY

E. W. SCRIPTURE.

The fundamental fact of binaural space is that when two component sounds heard by the two ears are perceived as a single sound the resultant sound is "localized in space." This "localization in space" is a direct experience of every person with binaural audition.

Aside from such problems as the influence of visual and muscular space on this localization, the fundamental problem is that of the localization of the resultant sound as dependent on the two components. The simplest sounds, tones, vary in pitch, intensity and duration. The only property which comes essentially into question in the case of the two components is that of intensity. The following paragraphs will present a hypothesis in respect to the law of localization as dependent on the intensity of the components.

The fundamental observation is as follows: When two component tones, e. g., from the telephones opposite the two ears, are heard as one tone, this tone is located in a certain direction in respect to the observer. When the intensities of the two components are equal, the resultant appears to be in the median plane, e. g., directly in front. As one of the components is weakened, the resultant appears to pass toward the side of the stronger one, finally reaching a line nearly opposite the ear—the auditory axis—and proceeding outward along it. The localization thus depends on the difference between the intensities of the components. Various observations lead me to believe that the following equations express this dependence.

Let I_R and I_L be the intensities of the right and left components, and let $d = I_R - I_L$ be the difference between the two intensities.

Let the plane in which the resultant lies contain a system of rectangular coördinates, with the origin in the median plane, the axis X identical with the acoustic axis, and the axis Y perpendicular to X; thus Y may lie anywhere in the median plane. Since the position of the sound with respect to these axes depends on the difference of intensity, we have $x = f(d)$ and, since there is a definite relation between y and x, $y = f(x)$.

In the experiments it is observed that when the two sounds are equal, i. e., $d = 0$, the resultant is located in the median plane, i. e., on the

axis Y at a certain distance m from the center. Thus for $d = 0$ we have $x = 0$, $y = m$. As the sound on the right is made louder we have $I_R > I_L$ and d positive; when the sound on the left is made louder d becomes negative. As one component becomes louder than the other, the resultant moves toward the side of the louder component; indicating the right side by + and the left by — we have $+x = f(+d)$ and $-x = f(-a)$. The resultant lies always on the positive side of the Y axis, which we can express by considering y as a function of the square of x, or $y = F(x^2)$.

The path described by the resultant sound appears to me to be a curve of the form given by the equation

$$y = me^{-\frac{x^2}{am}}$$

where m is the value for $x = 0$ and a is a constant of proportionality.

On the basis of these observations and considerations I venture to make the following two hypotheses: 1. that the distance right or left of the median plane is proportional to the difference between the intensities of the two components, i. e., $x = cd$, when c is the factor of proportionality; 2. that the relation between the distance from the median plane and the distance from the auditory axis is expressed by

$$y = me^{-\frac{x^2}{am}}$$

where m is the distance of the sound when $x = 0$ (i. e., $d = 0$), and a is a proportionality factor.

The values m and a depend on certain properties of the sounds used, but mainly on the absolute intensity. Sometimes the sound appears to remain always in the auditory axis,

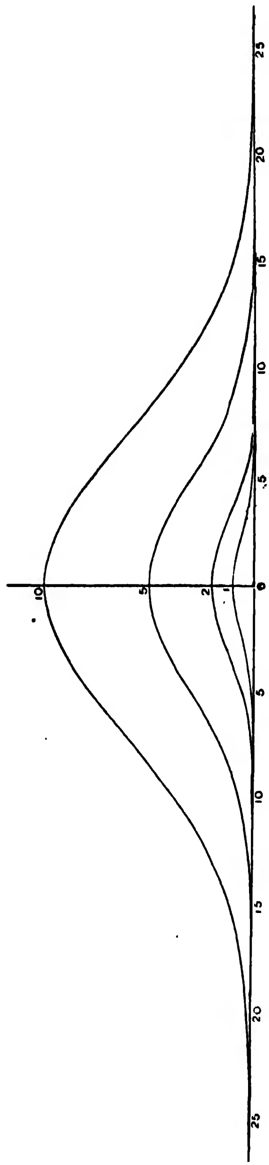


FIG. 22.

in which case $m = 0$. A series of curves for different values of m is shown in Fig. 22. The values from which the curves were plotted are given in the table.

TABLE OF $y = me^{-\frac{x^2}{am}}$ FOR $a = 10$.

x	$p_{10} = \frac{x^2}{100}$	$m = 10$: $r_{10} = p_{10} \times 0.4343$	$m = 10$: $1.0000 - r_{10} = \log y_{10}$	y_{10}	$p_5 = \frac{x^2}{50}$	$m = 5$: $r_5 = p_5 \times 0.4343$	$m = 5$: $0.6990 - r_5 = \log y_5$	y_5	$p_2 = \frac{x^2}{20}$	$m = 2$: $r_2 = p_2 \times 0.4343$	$m = 2$: $0.3010 - r_2 = \log y_2$	y_2	$p_1 = \frac{x^2}{10}$	$m = 1$: $r_1 = p_1 \times 0.4343$	$m = 1$: $0.0000 - r_1 = \log y_1$	y_1
0	0.00	0.0000	1.0000	10.000	0.00	0.0000	0.6990	5.000	0.00	0.0000	0.3010	2.000	0.0	0.0000	0.0000	1.000
1	0.01	0.0043	0.9957	9.902	0.02	0.0087	0.6903	4.901	0.05	0.0217	0.2793	1.904	0.1	0.0434	0.9566—1	0.905
2	0.04	0.0174	0.9826	9.607	0.08	0.0347	0.6643	4.616	0.20	0.0869	0.2141	1.637	0.4	0.1737	0.8263—1	0.670
3	0.09	0.0393	0.9607	9.135	0.18	0.0785	0.6205	4.174	0.45	0.1964	0.1046	1.273	0.9	0.3927	0.6073—1	0.405
4	0.16	0.0695	0.9305	8.521	0.32	0.1389	0.5660	3.681	0.80	0.3480	0.9530—1	0.897	1.6	0.6949	0.3050—1	0.202
5	0.25	0.1086	0.8914	7.787	0.50	0.2172	0.4718	2.956	1.25	0.5430	0.7580—1	0.573	2.5	1.0857	0.9141—2	0.082
6	0.36	0.1564	0.8435	6.974	0.72	0.3127	0.3763	2.379	1.80	0.7817	0.5193—1	0.331	3.6	1.5635	0.4365—2	0.037
7	0.49	0.2128	0.7872	6.126	0.98	0.4256	0.2734	1.878	2.45	1.0640	0.2370—1	0.173	4.9	2.1280	0.8720—3	0.007
8	0.64	0.2779	0.7221	5.273	1.48	0.5559	0.1431	1.391	3.20	1.3897	0.9113—2	0.082				
9	0.81	0.351	0.6482	4.447	1.62	0.7036	0.9954—1	0.989	4.05	1.7589	0.5421—2	0.035				
10	1.00	0.4343	0.5657	3.679	2.00	0.8686	0.8305—1	0.677	5.00	2.1715	0.1295—2	0.013				
11	1.21	0.5255	0.4744	2.981	2.42	1.0509	0.6481—1	0.445	6.05	2.6275	0.6735—3	0.005				
12	1.44	0.6254	0.3746	2.369	2.88	1.2508	0.4492—1	0.281								
13	1.69	0.7340	0.2660	1.845	3.78	1.4679	0.2310—1	0.170								
14	1.96	0.8512	0.1488	1.409	3.92	1.7026	0.9964—2	0.099								
15	2.25	0.9771	0.0229	1.069	4.50	1.9543	0.7447—2	0.056								
16	2.56	1.1112	0.8888—1	0.774	5.12	2.2236	0.4754—2	0.030								
17	2.89	1.2550	0.7440—1	0.554	5.78	2.5100	0.1890—2	0.015								
18	3.24	1.4071	0.5919—1	0.391	6.48	2.8142	0.8848—3	0.009								
19	3.61	1.5778	0.4222—1	0.264												
20	4.00	1.7372	0.2628—1	0.183												
21	4.41	1.9153	0.0847—1	0.122												
22	4.84	2.1020	0.8980—2	0.079												
23	5.29	2.2974	0.7026—2	0.050												
24	5.76	2.5016	0.4984—2	0.032												
25	6.25	2.7144	0.2856—2	0.019												
26	6.76	2.9359	0.0641—2	0.012												
27	7.29	3.1650	0.8350—3	0.007												

There still remains the fact that the plane of XY (which we have considered) may be in any position around the auditory axis; thus the sound may pass in front of, above, behind or below the head, or in any intermediate position. Four such positions are shown in Fig. 23. To fully

FIG. 23.

define the apparent position of the sound, we must introduce a system of coördinates in which X is the auditory axial line through the head, Z is a line perpendicular to this at the central point in the head and extending in the direction which the subject considers to be directly in front, and Y is perpendicular to both X and Z. We thus have $x = cd$ as before. Then in a case where the sound lies in the XZ plane to the front we have $y = 0$ and

$$z = me^{-\frac{x^2}{am}}$$

When the sound is upward in the XY plane we have $x = 0$ and

$$y \; me = {}^{-\frac{x^2}{am}}$$

The complete relation is expressed by

$$x = cd,$$

$$y = me^{-\frac{x^2}{am}} \cdot \sin a,$$

$$z = me^{-\frac{x^2}{am}} \cdot \cos a,$$

where a is the angle of elevation above the plane XZ.

This series of hypotheses agrees with the facts reported by MR. MATSUMOTO in the preceding pages, but cannot be proven until the experiments are repeated with tones of carefully measured intensities. I cannot say that I expect them to be confirmed just as they stand; I propose them simply as an attempt to give definite form to our notions of one of the laws of binaural localization.

THE SIZE-WEIGHT ILLUSION AMONG THE BLIND.

BY

JAMES F. RICE, M.A.

It is a well known fact that when two objects of equal weight but different size are lifted, the smaller appears heavier than the larger. The phenomenon has been made the subject of experiment in various ways.[1]

It was suggested to me that some experiments on the blind might be of interest. The experiments were performed at the New York Institution for the Blind. They were carried out under the direction of the Yale laboratory with the suggestion-blocks formerly used by Dr. SEASHORE. Many suggestions in regard to the details of the experiments were received from Dr. SEASHORE personally.

APPARATUS.

The apparatus consisted of two sets of cylindrical blocks 31^{mm} in length. Each set consisted of 17 blocks. Set *A* varied in size and had a uniform weight, while Set *B* varied in weight and had a uniform size. The blocks in Set *A* varied in diameter according to a geometric series in which the regular increment is one-tenth. Those in Set *B* were arranged in arithmetic series according to weight with a successive difference of 5^g.

In the following account the blocks will be dintisguished by the names *A* and *B* with their respective numbers in the series.

---

[1] FECHNER, *Ueber die Contrastempfindung*, Ber. d. k.-sächs. Ges. d. Wiss., math. phys. Cl., 1860 XII 76.

MUELLER and SCHUMANN, *Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte*, Archiv. f. d. ges. Physiol. (Pflüger), 1889 XLV 37.

CHARPENTIER, *Analyse de quelques éléments de la sensation de poids*, Archives de Physiol., 1891 (5) III 122.

DRESSLAR, *Studies in the psychology of touch*, Am. Jour. Psych., 1894 VI 313.

FLOURNOY, *De l'influence de la perception visuelle des corps sur leur poids apparent*, L'Année Psychol., 1894 I 198.

GILBERT, *Researches on the mental and physical development of school-children*, Stud. Yale Psych. Lab., 1894 II 43–45, 59–63.

PHILIPPE and CLAVIÈRE, *Sur une illusion musculaire*, Revue Philos., 1894 XL 674.

VAN BIERVLIET, *La mesure des illusions de poids*, L'Année Psychol., 1895 II 79.

GRIFFING, *On the sensations of pressure and impact*, Psychol. Rev., 1895 II Suppl. I.

SCRIPTURE, *Remarks on Dr. Gilbert's article*, Stud. Yale Psych. Lab., 1894 II 102.

The blocks of Set *A* were of a constant weight, 80^g, and of diameters in millimeters as follows, beginning with the smallest: 20.0, 22.0, 24.2, 26.6, 29.3, 32.2, 35.4, 39.0, 42.9, 47.2, 51.9. 57.1, 62.8, 69.1, 76.0, 83.6, 91.9.

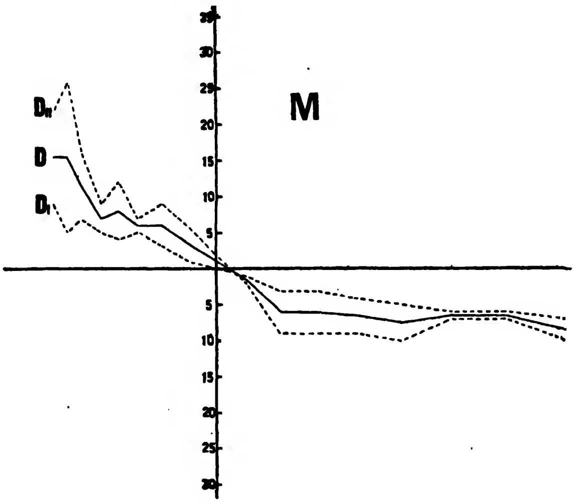


FIG. 24.

The blocks of Set *B* were of a constant diameter, 42.9^{mm}, and of weights in grams as follows, beginning with the lightest: 40, 45, 50, 55, 60, 65, 70, 75, 80, 85, 90, 95, 100, 105, 110, 115, 120.

It is to be observed that the uniform weight for Set *A* is the same as the weight of *B* (9), the middle block in Set *B*; and the uniform size in Set *B* is the size of *A* (9), the middle block in Set *A*.[1]

The observer placed himself by the table, on which the blocks were arranged in order, in such a position that by moving back and forth he could lift any block from its place in Set *B* and still retain approximately the same angle of the arm and hand. He was requested to select for each block in Set *A* a corresponding one in Set *B*, by taking one at a time from *A* and placing it by the side of successive blocks in *B* with which he wished to compare it, lifting one at a time until he found the one in *B* which he thought had the same weight as the one from *A*.

One series of tests *D* was made in which the size of the weights was learned by grasping the curved surface of the block; a second series of tests *E* was so made that the observer could only judge of the size from

[1] SEASHORE, as before; SCRIPTURE, New Psychology, Fig. 65, London 1897.

the area of pressure when the block was placed gently upon the palm of his hand. All the *A* blocks were used in each series. Each series of tests was made ten times, and, to eliminate as far as possible the error of prejudice, the equivalent *B* for each *A* block was approached five times from above, five times from below. That is, in five of each series an *A* block was compared first with a *B* that was very perceptibly heavier and then with the *B* of next lower weight until apparent equality was reached. In the other five the steps were from the perceptibly lighter.

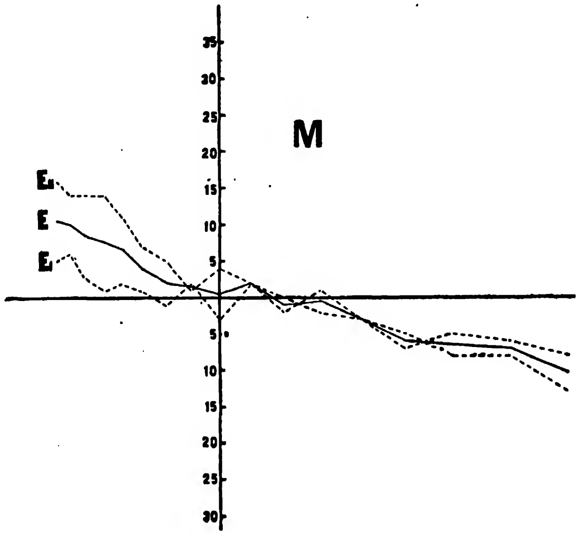


Fig. 25.

To eliminate the error of sequence, the observer lifted the blocks in the orders *A*, *B*, *B*, *A*, and *B*, *A*, *A*, *B* alternately. The position of the blocks was reversed after each trial, so that the observer's judgment was not affected by the varying sensations caused by the slight movement of the forearm to the right or left. By this exchange of position there was also to a slight extent avoided the fixing of the observer's attention upon the *A* with which the several *B*'s were being compared; his judgment was, without explicit reference to either block as a standard, merely a judgment of equality. In case of a perceived difference he indicated which was the heavier.

Subjects.

It is desirable for experiments on the psychology of the blind that the subject should have been totally blind from birth. It has been held, in-

deed, that with the congenitally blind we should class as competent observers those who became blind during their first year,[1] and many who have studied the blind have included in their observations those who had lost their sight as late as the seventh year.[2] Obviously the mental life of those whose experience includes light sensations cannot be identical with that of those totally blind from birth. The assumption that the conditions are similar cannot be established until the psychology of the congenitally blind, who have never seen light, has been first studied. The subjects in the experiments here described have been blind from birth and have never seen light.

M is a college graduate and university professor; he is a mathematician of international reputation. *O*, his brother, is in business in New York. They are both graduates of the New York Institution for the Blind.

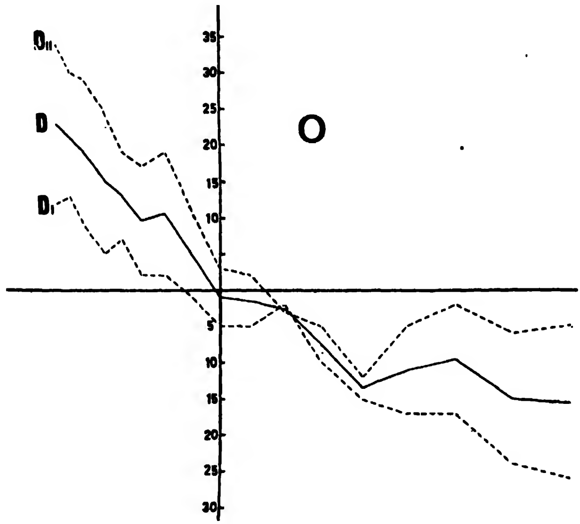


FIG. 26.

M was from the start quite aware of the illusion, and though he was kept in ignorance of the purpose of the tests, he repeatedly spoke of the impossibility of correct estimates of weight when the objects compared were of different sizes. It was his opinion that of the two tests the second, i.e., with the weights on the palm of the hand, was the least accurate. His custom had always been to compare weights by grasping in quick

[1] HELLER, *Studien zur Blinden-Psychologie,* Phil. Stud., 1895 XI 252.

[2] HOCHEISEN, *Ueber den Muskelsin bei Blinden,* Zt. f. Psych. u. Phys. d. Sinn., 1893 V 239.

succession the objects to be judged, and dropping them, if small, from one hand to the other, or by weighing them upon the tips of the fingers, as in the case of coins. In the methods of these experiments he had no previous experience.

O was also fully aware of the illusion. He, like *M*, considered the third series of judgments the least satisfactory, and expressed the same

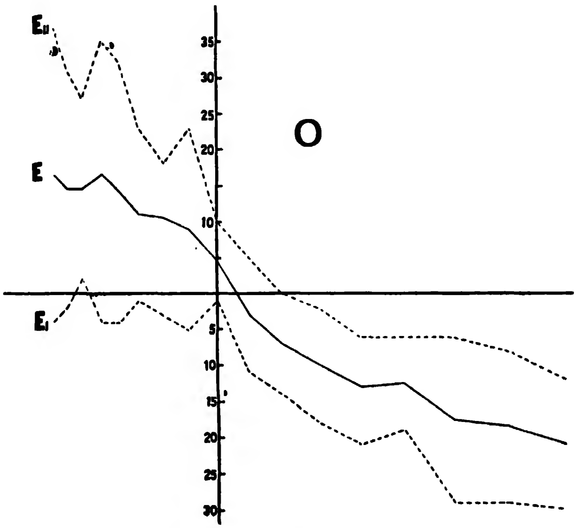


FIG. 27.

preference for grasping rather than mere lifting upon the palm. *O* thought that size was less diverting in the first series than in the second.

EXPERIMENTS.

(1) The first series of tests was that in which the knowledge of size was gained through the muscle sense, corresponding to SEASHORE's "fourth series, H, muscle sense." An *A* and a *B* block, resting on end upon a soft pad, were lifted in succession, being grasped around the circumference by the thumb and middle finger of the right hand. If the weights were judged unequal, the *B* was replaced by another of the same set. Because the observer did not have to make any choice of *B*'s, but to consider only two blocks at a time (selected by the one conducting the experiment), he could fix his attention upon the question of equality of weight undistracted by the knowledge of the number of blocks that might possibly be compared.

(2) In another set of experiments the block was laid on the palm of

hand; this gave an idea of the size, the height being known to be constant. This corresponds to SEASHORE's "fourth series, I, touch."

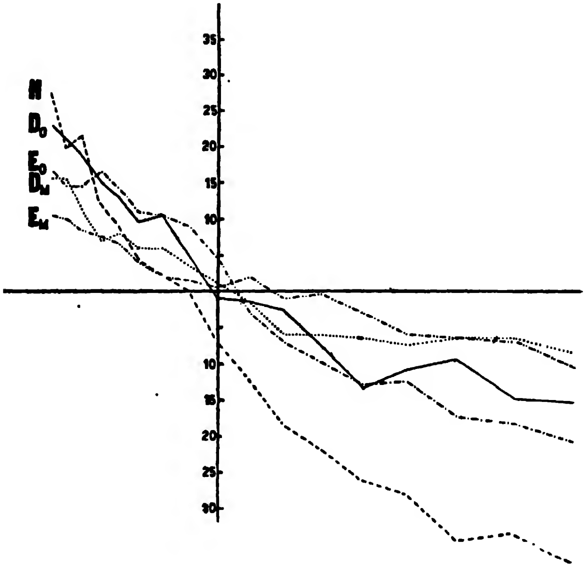


FIG. 28.

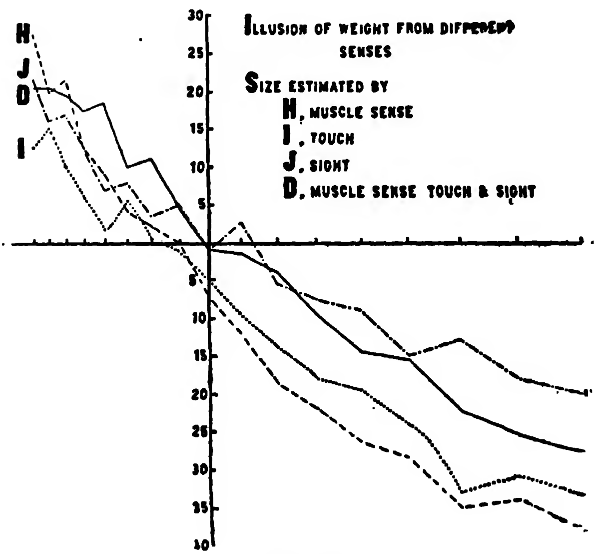


FIG. 29.

The results are given in the table. Each result in the columns D_I and D_{II} is the average of 5 original measurements; the probable error for these results never exceeds 4% and is generally less than 2%. The results in the table are indicated graphically in Figures 24, 25, 26, 27. Figure 28 shows the average results for both observers together with the curve H of Dr. SEASHORE's experiments.

	M						*O*					
C	D_I	D_{II}	D	E_I	E_{II}	E	D_I	D_{II}	D	E_I	E_{II}	E
−22.9	+9	+22	+16	+5	+16	+11	+12	+34	+23	−4	+37	+17
−20.9	+5	+26	+16	+6	+14	+10	+13	+30	+22	−2	+31	+15
−18.7	+7	+16	+12	+3	+14	+9	+9	+29	+19	+2	+27	+15
−16.3	+5	+9	+7	+1	+14	+8	+5	+25	+15	−4	+35	+16
−13.6	+4	+12	+8	+2	+11	+7	+7	+19	+13	−4	+32	+14
−10.7	+5	+7	+6	+1	+7	+4	+2	+17	+10	−1	+23	+11
−7.5	+3	+9	+6	−1	+5	+2	+2	+19	+11	−3	+18	+8
−3.9	+1	+6	+4	+2	+1	+2	−1	+11	+5	−5	+23	+9
0	0	+2	+1	−3	+4	+1	−5	+3	−1	−1	+10	+5
+4.3	−1	−2	−1	+2	+2	+2	−5	+2	−1	−11	+5	−3
+9.0	−3	−9	−6	0	−2	−1	−2	−3	−5	−14	0	−7
+14.2	−3	−9	−6	−2	+1	0	−10	−5	−7	−18	−2	−10
+19.9	−4	−9	−6	−3	−3	−3	−15	−12	−13	−21	−6	−13
+26.2	−5	−10	−7	−5	−7	−6	−17	−5	−11	−19	−6	−12
+33.1	−6	−7	−6	−8	−5	−6	−17	−2	−9	−29	−6	−17
+40.7	−6	−7	−6	−8	−6	−7	−24	−6	−15	−29	−8	−18
+49.0	−7	−10	−8	−13	−8	−10	−26	−5	−15	−30	−12	−21

M, *O*, subjects.

C, number of millimeters by which the diameter in Set *A* differed from that in Set *B* (having a weight of 80g).

D_I, D_{II}, D, number of grams by which the estimated weight of the block in Set *A* differed from its true weight, the block being grasped.

E_I, E_{II}, E, same as D_I, D_{II}, D, the block resting on the skin.

D_I, E_I, experiment begun with the lighter blocks.

D_{II}, E_{II}, experiment begun with the heavier blocks.

D, E, averages of D_I and D_{II}, and of E_I and E_{II} respectively.

For the sake of comparison the curves obtained by SEASHORE for the same senses in the seeing are reproduced in Fig. 29 from Fig. 5 of SEASHORE's monograph.

CONCLUSIONS.

The results seem to justify the following conclusions:

1. The size-weight illusion obtains among the blind.

2. It follows the same general law as among the seeing, but is not so great either for the muscle sense or for touch as for the same senses among the seeing.

3. The illusion tends towards the side on which the weight is approached.

CEREBRAL LIGHT.[1]

BY

E. W. Scripture.

In darkness or with closed eyes we can always see irregular forms of light in our visual field. These forms are of various kinds, series of waves, successive rings that spread and break, etc. In addition to these definite figures there is always more or less definite irregular illumination over the whole field. These phenomena are generally called "the retinal light" or the "*Eigenlicht* of the retina." They are usually supposed to arise from chemical changes going on in the retina. I wish to record some observations that apparently prove them to be cerebral and not retinal processes.

1. With closed eyes there is only one illuminated field, not two, as there should be from the two retinas if the light were retinal. Two retinal figures might appear as one under the conditions: (*a*) of suppression of one field, which is not the case here, because it is impossible to keep one field suppressed for many minutes, whereas I have watched the retinal figures in uninterrupted continuance for a long time; (*b*) of perfect identity of form, which is hardly a possible supposition in the case of these irregular, volatile, chemical phenomena; (*c*) of sufficiently similar construction for union by stereoscopic vision, which also is not the case, as there is no relief-effect in the picture.

2. The figures do not change in position when the eye is moved. They are localized in front and remain in the same place, even if the eyes are directed to one side. I find, however, that if the eyes are turned to a new position and kept there, the central figure (a spreading violet circle with a phosphorescent rim) will soon afterwards follow the movement; there is thus a tendency for this figure to occupy the spot of sharpest vision.

3. The figures do not change in location when the eyes are displaced. When the eyes are looking at some definite object, e. g., this page, a pressure of the finger on one of them will cause the page apparently to move. This is true whether the other eye is open or closed. Likewise, if an after-image is obtained, it will move upon pressure of the eyeball. The pressure displaces the eyeball and changes the projection of the re-

[1] This account was first published in Science, 1897 N. S. VI 138.

tinal picture. This displacement does not occur with "retinal light." I have repeatedly observed these figures and have manipulated the eyeballs; I have found that they are not in the slightest degree affected by the manipulations. In order to avoid all possibility of errors of observation, I have made the experiments in a series alternately with eyes open and eyes closed. With the eyes open I observed a dimly illuminated window; with them closed I saw the "retinal" figures. The former always followed the displacements, the latter never.

These observations are, I believe, sufficient to establish the proposition (which I have not seen elsewhere) that the phenomena of vision usually known as "retinal light" and "retinal figures" are not originated in the retina, but in the brain. They should, therefore, be termed "cerebral light" and "cerebral figures."

The following hypothesis seems also justified. The cerebral light is located in those higher centers of the brain which are connected with visual memories and imaginations. While watching the cerebral figures I find that my visual memories or phantastic figures appear in the midst of the cerebral light and frequently cannot be distinguished from them. The close connection of these cerebral figures with the contents of dreams has been repeatedly noticed by JOHANNES MUELLER and a series of later observers. There is also the possibility that the hallucinatory visions produced by hashish, mescal and other drugs may be simply modifications of this cerebral light.

RESEARCHES ON MEMORY FOR ARM-MOVEMENTS.

BY

E. W. SCRIPTURE, W. C. COOKE AND C. M. WARREN.

In the arm-space board[1] a wooden scale carries along its upper edge, a small glass rod. At the zero point in the middle there is a fixed metal plate. On each side there is a movable slide carrying an adjustable pointer. Before the experiments the pointers are pushed forward as far as possible.

The apparatus is placed on a table with the scale away from the subject. The subject, seated with eyes closed or covered, places his forefingers against the zero-plate, one on each side.

The experimenter moves up the two slides to the fingers till they press gently. The pointers strike the zero-plate and are pushed back automatically. This eliminates the errors due to the widths of the finger, as all readings are to be taken from the end of the pointer.

The subject places himself directly in front of the zero-mark and closes his eyes. The experimenter places the left-hand (referring to the subject) slide at a certain distance, d. The right-hand slide is moved out of the way. The subject moves his left fore-finger evenly outward till it strikes the slide, and then returns it to zero. The experimenter quietly moves the slide out of the way, and after an interval of t seconds the subject moves his finger again till it seems to be in the same place as before. The experimenter now moves the slide up till it touches the finger and reads the record at the end of the pointer. The tenths of a centimeter are estimated by the eye. The result in millimeters is placed in the record blank.

In a set of experiments carried out by MR. COOKE in 1896–97 on four college students as subjects, the distances 100^{mm}, 300^{mm} and 500^{mm} were investigated for the intervals 2^s, 10^s and 20^s. The constant and probable errors were calculated in the following way. If $a_1, a_2, \cdots, a_n$ are the observations for given values of t and d, we have for the average

$$a=\frac{a_1+a_2+\cdots+a_n}{n},$$

from which we obtain the constant error $C=a-d$.

[1] SCRIPTURE, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 97; SCRIPTURE, New Psychology, 187, London 1897.

For. the variations, or errors, we have $v=a_1-a$, $v_2=a_2=a, \cdots$, $v_n=a_n-a$; from which we obtain the probable error for a single measurement by the shorter method

$$P=0.8\frac{|v_1|+|v_2|+\cdots+|v_n|}{n-1}$$

where $|v|$ means that the sign of v is disregarded. The results for four subjects, A, B, C and D, are given in Table I; five experiments were made on each point.

TABLE I.

Distance.	Time.	A		B		C		D	
		C	P	C	P	C	P	C	P
100 mm.	2ˢ	+ 11	5.8	+ 11	8.9	+ 2	3.0	− 2	8.9
	10	+ 22	12.1	+ 10	8.9	− 10	6.5	− 4	7.1
	20	+ 15	12.5	+ 15	8.9	− 16	2.4	− 4	6.4
300 mm.	2ˢ	+ 6	12.8	+ 10	8.9	− 5	11.2	+ 9	8.9
	10	+ 6	24.6	− 13	11.9	− 30	13.6	− 8	16.9
	20	− 5	14.5	− 13	13.0	− 27	17.6	− 12	24.0
500 mm.	2ˢ	+ 6	7.4	+ 7	8.3	− 7	12.9	− 5	7.8
	10	− 2	8.1	− 8	7.5	− 1	9.4	− 15	14.8
	20	− 1	7.3	− 5	7.5	+ 6	11.0	− 24	21.7

The table seems to justify the conclusion that the law according to which the constant error varies in relation to the elapsed interval, is a purely individual matter, as was first pointed out by SCRIPTURE (New Psychology, 189).

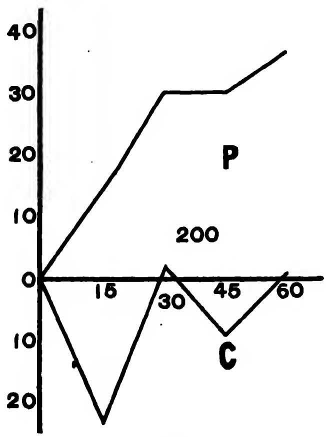


FIG. 30.

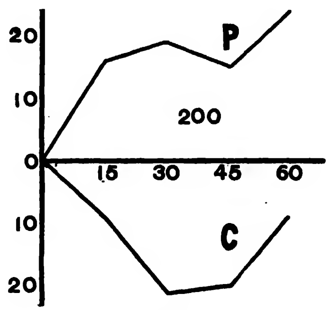


FIG. 31.

The probable error also seems to follow no general law, although there are more cases in which it increases with the increase in interval than in which it decreases or remains constant or fluctuates.

For comparison with these experiments on untrained observers another set was undertaken by Mr. WARREN in March, 1898, in which the subject was the laboratory janitor, A. FISHER, a trained observer[1] with no interest in the results. The constant error was calculated as before, but the probable error was derived by the more accurate formula

$$P = \tfrac{2}{3}\sqrt{\frac{v_1^2 + v_2^2 + \cdots + v_n^2}{n - 1}}.$$

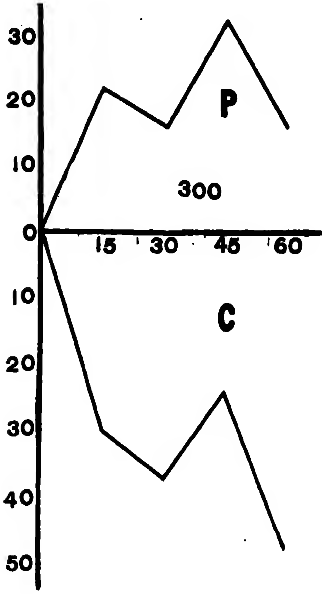


FIG. 32.

The results for the intervals 15^s, 30^s, 45^s and 60^s and for the two distances 200^{mm} and 300^{mm} are given in Table II. The first division for 200^{mm} and that for 300^{mm} give the results when the experiments were made in the following order: 15^s — 200, 300, 300, 200; 30^s — 200, 300, 300, 200, etc. The other experiments on 200^{mm} were made on successive occasions, the distance being constant but the intervals of time being varied irregularly, thus 200^{mm} — 15, 60, 45, 30, etc. The values of the table are expressed in Figures 30, 31 and 32.

TABLE II.

	200^{mm}			200^{mm}			300^{mm}		
Time.	*C*	*P*	*n*	*C*	*P*	*n*	*C*	*P*	*n*
15^s	—23	17.3	17	— 9	16.3	15	—30	21.8	17
30	+ 2	30.2	17	—21	18.5	15	—37	15.9	17
45	— 9	29.5	12	—20	15.1	15	—24	31.5	12
60	+ 1	37.2	12	— 9	23.6	15	—47	15.7	12

These experiments seem to justify the conclusion that even for the same person the law of the constant error changes with the distance and with the method of the experiment. The probable error, however, is quite evidently an increasing function of the time-interval; the experiments are, however, not extensive enough to justify an attempt at determining its law.

[1] Stud. Yale Psych. Lab., 1896 IV 24.

PRINCIPLES OF LABORATORY ECONOMY.

BY

E. W. SCRIPTURE.

One of the most difficult portions of the work of the experimental psychologist is the development of the technical methods required by the science. One set of these methods comprises those involved in managing a laboratory. The following results of an experience of four years as a student in the laboratories of Leipzig and Worcester and of six years as director of the Yale laboratory may be of value to others. The material comprises the substance of what I am accustomed to say on this subject in a few lectures of the regular technical course for specialists in psychology.

GENERAL PLAN.

Two distinct kinds of work fall upon the laboratory; they may with fairness be characterized as: 1, college work; 2, university work.

College work.

In a college the aim is to provide an outline-knowledge of the subject sufficient for general culture. Research or advanced courses are quite out of the question.

In a small college the resources must necessarily be limited. With an appropriation of a few hundred dollars the instructor can get along with one or two rooms, an equipment of tables and chairs, apparatus for time-records, sight, hearing, etc. What can be done on the subject of the senses at small expense is shown in SANFORD'S Course in Experimental Psychology, Part I, Boston 1898. However, it must be borne in mind that the book covers only a portion of the science and it is very important that the instructer should not give the impression that this is all of psychology. In a laboratory with moderate means the mechanical skill and ingenuity of the instructor come especially into play. With a fair supply of tools the trained man can give a more interesting and valuable course at a small expense than an unskilled man with far greater facilities. The frequent inquiries concerning the amount requisite for starting a college laboratory may be met with the statement that it all depends on the man employed. Success with only \$300 or \$500 a year is possible for a skillful man who has had a thorough training, and

who has had experience as an assistant in a larger laboratory. With $500 at the start, about $200 should be put aside for running expences and for unforeseen contingencies. For $50 a good lathe and a number of small tools can be obtained. If the electric current is available in the building, a hand-feed arc lamp can be imported for about $5, a resistance coil for $2.50, a pair of condensing lenses for $1.50, a simple objective for $1; the rest of the lantern can be made as indicated in these Studies, 1896 IV 84. If electricity is not available, a different luminant may be used. An English oil-lantern need not be expensive; an acetylene lantern is not excessively costly and is very successful; an oxy-hydrogen lantern for compressed gases, though more costly, is very satisfactory, especially if fitted with the LINNEMANN burner for zircon-plates. The slides can be made by the instructor or by an interested student; a very large number of experiments can be performed with the lantern instead of with "demonstration" apparatus. For a few dollars each the following articles may be obtained: color wheel, Kolbe's color cylinders, color discs, Bradley's pseudoptics, stereoscopes, stereoscopic diagrams, etc. More costly but necessary pieces are: recording drum, tuning fork, telegraph keys, batteries, etc.

For the larger colleges a more elaborate equipment is necessary. Comparisons are constantly drawn between the various departments, and merely as a matter of self-preservation the psychological laboratory must offer courses equal in attractiveness and value to those of physics, chemistry and biology. A lecture-room with at least a single lantern and a satisfactory equipment of demonstration apparatus should be provided. The elementary laboratory course should be followed by a carefully planned course in psychological measurements. Part of the course in measurements given at Yale is printed in these Studies, 1896 IV 89–139. The appropriation and yearly income of the laboratory must be quite considerable, if the lectures are to be on an impressive scale and the two laboratory courses are to be really valuable.

University work.

The university student chooses to study psychology either because he will need it in connection with philosophy, pedagogy or medicine, or because he expects to be a psychologist himself. The laboratory should furnish instruction for both classes of students.

In the first place a general lecture-course should be provided with full demonstrations of all the most important methods and results in all the fields of psychology. The apparatus required must necessarily be of a distinctly high grade. The university student demands the best in-

struction. Moreover, if the other departments, such as physics can show better, brighter and more numerous pieces of apparatus the students are apt to draw disparaging conclusions. The students are no longer a "class" to be taught; they are an "audience" that must be led. When a speaker, whether on account of himself or on account of meager equipment, loses the interest or the respect of his audience, the instruction ceases to be effective.

Laboratory courses, like those already mentioned, should also be given. For the special students in psychology a course of lectures on the theory of statistics and measurements should also be given in connection with practical exercises in the laboratory. These exercises should be most carefully planned to show the development of the final methods of measurement from the fundamental operations. For example, the subject of time might begin with timing and regulating a clock; thereupon would follow the timing of a tuning fork, the determination of the errors of markers, keys, etc. When this has been learned simple reactions can be measured under the various conditions of stimulation, attention, etc. Finally, the more complex processes are to be recorded. Similar methods could be followed for sight, hearing, volition, emotion, etc.

The object of the laboratory instruction is often misunderstood. It is not to teach facts of psychology; these should be treated in a lecture-course. The objects are: (1) the cultivation of the powers of introspective and external observation; (2) the development of dexterity in carrying out psychological experiments and measurements; (3) training in the computation and adjustment of measurements. Students who make a specialty of psychology should also be trained in the construction and care of apparatus, in machine-design, use of tools, lathe work, vise work, etc.

Another department of university work lies in research. The art of research is probably the most difficult thing to teach. Some persons have the notion that anybody can successfully undertake researches with no preliminary training. Research is an *art*, as much an art as painting or singing. Any one can daub paints, sing tunes and investigate the mind, but the results differ from a painting by Correggio, a song by Patti, or an investigation by Helmholtz. The director of a laboratory for research must be a man with an inborn tact at overcoming obstacles of apparatus and in extricating himself from intricacies of method. He must be a man of vivid imagination; just as a painter must compose and constantly modify the ideal picture in his mind's eye, so the scientist must outline, invent and modify mentally the whole apparatus and method employed before he begins the actual work. Just as a painter must acquaint him-

self with the minutest details of preparing and mixing paints, of perspective, shading, varnishing, etc., so must the psychological investigator have in mind just as many kinds and combinations of apparatus, manipulations and methods as he can possibly learn. This is the ideal which the special student should always have in mind. The young investigator should go through a preliminary apprenticeship in assisting more advanced workers, and should prove himself a well qualified man before attempting to conduct work.

Building.

If possible the laboratory should be situated in a building back from the street, preferably in the suburbs. If it is in a building with other departments it should have the top floor, and the smaller rooms for observation should be at the back. Just as freedom from shaking is the indispensable condition for many physical experiments, so freedom from noise is the fundamental requisite for the successful prosecution of many psychological investigations. Of course, where only demonstrational work is required such conditions are unattainable and unnecessary.

In regard to the general plan of a scientific building reference may be made to Durm's Handbuch der Architektur, IV. Theil, 6. Halb-Band, 2. Heft: *Gebäude für Erziehung, Wissenschaft und Kunst.*

Lecture-room.

One of the first cares of the instructor will probably be the lecture-room. Although one like that to be described will seldom be attainable, the main requirements are the same for most cases and they can be met in ways as nearly like the ideal ones as possible. All seats in the room should command a good view of the experiment table. In a small institution a large table in an ordinary lecture-room would be sufficient. With large classes, however, the seats must rise toward the back; the rise should be about 12^{cm} at the second row with a regularly increasing rise for each succeeding row. If the seats are arranged in curves, a rather flat hyperbola ought to be chosen. Before the windows black shades or shutters for darkening should be placed. The many elaborate arrangements in use are generally very costly; recourse is generally had to common spring rollers. For further suggestions see Weinhold's Physikalische Demonstrationen and Frick-Lehmann's Physikalische Technik.

The ceiling and walls of the room should be as white as possible. The researches in school-hygiene have shown that dark colors, panelings, rows of blackboards and any other arrangement that lessens the diffusion

of light must be most scrupulously avoided.[1] The daylight should come from the left and back. Artificial illumination should be indirect or semi-indirect; that is, the light from the luminous body should not fall directly on the books of the students, but should be diffused by being first sent against the ceiling or by passing through translucent globes.[2]

The method of completely indirect illumination can be perhaps best used in the case of an arc light. The reflector beneath the lamp hides it completely from view and sends the light to the ceiling, whence it is dispersed around the room. The ceiling should be newly whitened each year; a special reflector, however, may be used above the lamp instead of the ceiling. In the combined method of illumination part of the light passes directly through a lower globe of translucent glass while part is reflected upward. For lecture-rooms the completely indirect illumination seems preferable.

Other sources of light, such as the AUER gas incandescent lamp, the electric incandescent lamp, etc., may be treated in like manner. Further details and literature may be found in the article by BAYR. A good account of arc-lighting is given in a pamphlet, *Ueber Bogenlampen für zerstreutes Licht*, published by the manufacturers, KÖRTING & MATHIESEN in Leutzsch, near Leipzig.

A fundamental piece of apparatus for the lecture-room is the lantern. A general sketch of the principles involved has been given in these Studies, 1896 IV 82–88; further details may be found in WRIGHT'S Optical Projection.

Another fundamental factor of the lecture-room is the experimenter's table. This should be a table, or rather a counter, about $5 \times 0.9 \times 0.9$ meters in size. Better than one table are two tables, each two meters long, placed endwise with an intervening space of about a meter which can be covered by a hinged leaf; this allows very tall apparatus to be set up on the floor between other apparatus. The space under the table is utilized for drawers and open closets where tools and apparatus of constant use are kept.

The fundamental rule in regard to the use of the table is: never allow any projection to be fastened to the top. Nails must not be driven in; they are sure to be forgotten and to get in the way at some critical moment, perhaps to overturn or scratch some valuable piece of apparatus. All nails and screws should be driven into blocks of wood which are fastened to the edge of the table by clamps. This rules applies to all

---

[1] BURNHAM, *Outlines of school hygiene*, Pedagog. Seminary, 1892 II 9.

[2] BAYR, *Ueber Beleuchtungsversuche, etc.*, Zt. f. Schulgesundheitspflege, 1898 XI 129.

tables in the laboratory except when, after trial, it is decided to set up some permanent piece of apparatus.

Workshop.

As the progress of psychology depends to a great degree on the way in which this department is managed, money invested in a proper equipment of tools will often bring better returns than if spent in ordering instruments from dealers. In fitting up this room we should take into consideration its purpose and that becomes clear when we take note of a few facts about apparatus. Apparatus in use industrially, such as electric motors, telegraph keys, batteries, etc., can be obtained readily and cheaply in America. On the contrary, apparatus that is in use only for scientific purposes can be obtaind here only at exhorbitant prices if at all, and owing to the lack of trained workmen it is generally of the poorest quality. All such instruments already in use, e. g., kymographs, tuning-forks, time-markers, etc., must be gotten directly from the European makers. In addition to these two classes there is apparatus for special purposes which must either be ordered from instrument makers or made in the laboratory workshop. The difficulty and delay in sending designs to Germany or France and the great disadvantage of not being able to supervise the various stages of construction are readily apparent. It is highly desirable in a large laboratory to set up a workshop and to provide a proper artisan to take charge of it. The work to be done will be the making of all special research-apparatus, also the repairing and testing of all instruments received from elsewhere. This course is the one followed by nearly all European laboratories of physics and physiology; the high wages and lack of skill of the American mechanic tend to throw the psychologist more on his own resources.

The size and equipment of such a workshop depend on the character of the instructor and the importance of the part played by research.

The first necessity of the workshop is sufficient light, both by day and by night; that is, there must be sufficient windows and lamps so arranged that there are no deep shadows into which the work in hand can happen to get. Nevertheless, no very bright or blinding lights are to be permitted. The room is preferably placed on the north side, otherwise the south windows must be well provided with curtains. The lamps are not to be placed in the middle of the room, but to be scattered over the walls and around the tables.

The most important and essential article in the workroom is the work-bench, which should be placed on the lightest side of the room, preferably in the corner. It is to be a very strong table of oak, or ash, which is

fastened by braces built into the wall, so that in filing, sawing and chiseling in the vise no noticeable shaking of the bench occurs. The size should be about $2^m \times 0.7^m$; the height should be 0.75^m to^m 0.80. The regular mechanic's height, 0.9^m, is to be reduced because this requires continual standing, which is a great hardship to persons of sedentary habits, such as will necessarily often be at work at the bench. The vise is to be placed at the right front corner of the table and, if possible, opposite the middle of the window. A smaller vise may be placed on the left-hand side of the table. Next in importance to the work-bench is the lathe. It is to be placed before the window on the north side of the room. There should also be a grindstone, with pulley in case power is at hand. In one corner there may be a forge beside which is a heavy anvil on an oaken block. By the eastern wall of the room there should be a carpenter's bench unless a separate room is to be given over to carpenter-work. Around the walls are the tool cases. The tools should, as far as possible, be put in straps or leaned up against grooved boards or laid in order on the shelves of the cases. The cases and the walls of the room are to be painted with a light color for sake of illumination. The floor is to be varnished; this protects it against too rapid wear, lightens the cleaning, and avoids the odors that arise from the absorption of gases by unvarnished floors. A stone floor is inadmissible. Further details in regard to the workshop may be found in LEHMANN's Physikalische Technik.

ELECTRICAL CONNECTIONS.

An essential for first-class work in psychology is the protection of the person experimented upon from all sources of disturbance. This generally involves the separation of the experimenter from the one experimented on and often involves the use of two or more separate rooms between which the only connection is by means of electric wires.

The experience of telegraphy and telephony has shown that the proper method of connection is by means of wires to a central station from each point involved. Wires should be brought from each room to a switchboard in some convenient place where they can be connected in any way desired. A switchboard on the plan of a telephone switchboard with 56 terminals is used at Yale; each wire is joined to a terminal and numbered. By means of flexible connectors any combination desired can be made and yet any unused wire left untouched. The wiring should be done strictly in accordance with the regulations of the board of underwriters, a copy of which can be obtained from the nearest fire insurance office. It is not advisable to use wire of a diameter less than 2.7^{mm} (No. 12, American gauge).

COLLECTION OF APPARATUS.

On account of moisture and dust the apparatus should not be kept in rooms frequently used. It is best to devote a special room to this purpose. The floor should be of wood and the cleaning should be done with a moist cloth. The cases for holding the instruments are to be furnished with glass doors which should be kept tightly closed. Each piece of apparatus should have a name or a number and a specified place in the case or cases; small gummed labels or jeweller's tags are useful for this purpose. Small cards with the names of the pieces are fastened on the shelves of the cases by card-pins.

Each piece of apparatus should be entered under its name in the inventory and the apparatus book. The latter should contain for each piece a complete record of its purchase, its use, its constants, where it is to be sent for repair, where to obtain parts, where to find literature on its use, etc. For this purpose a regular letter file can be used; all the data—bills, printed descriptions, etc.—can be filed with the rest of the account.

ISOLATED ROOM.

To obtain the quiet necessary for most psychological investigations an isolated room may be provided, where the person experimented upon can be kept indefinitely in perfect external darkness and quiet. A description of the Yale room and suggestions for improvement may be found in these Studies, 1893 I 271, 1896 IV 16, and in SCRIPTURE'S New Psychology, 136, London 1897.

OTHER ROOMS.

The other rooms will vary so much in arrangement with the construction of the laboratory that little can be said definitely. I would suggest: 1. an optical room with black walls, heliostat window and photometer arrangements; 2. a time room with clock, chronograph, chronoscope and other equipment; 3. a photographic room.

SELECTION OF APPARATUS.

Let us suppose that the appropriation has been obtained and that the director has, after carefully planning an ideal laboratory, made up his mind to adjust himself to the actual situation. The next thing to do is to select apparatus. For this purpose he should obtain catalogues from the chief makers and also lists of the apparatus possessed by other laboratories. The catalogues should be carefully studied. The director must at any time know just where to obtain each piece at the lowest price for the best quality. For example, electrical tuning-forks will

be described and illustrated in 15 or 20 catalogues, whereas there are not more than two places from which they can be bought to the best advantage. The list of needed apparatus is finally made out; when added up, it will be found to be four or five times the entire appropriation. A process of selection must now be instituted. I venture to propose a rule, almost self-evidently proper, yet often neglected: select those pieces that for each dollar of cost will bring a maximum return in time of use and in results obtained. For example, let us suppose it necessary to choose between the Ellis harmonical ($65), a color-apparatus for excentric parts of the retina ($15), and a kymograph ($175).

It would hardly be justifiable to occupy more than half an hour of a lecture-course in experiments for which the harmonical can be used. Let us suppose that the class includes forty persons. The time-value of the harmonical can then be said to be $\frac{1}{2} \times 40 = 20$ efficient hours per year. Let the deterioration and interest on investment on all the instruments be placed at 10% per year. The cost of the harmonical is thus $6.50 for 20 hours, or about $0.33 per hour per student. The color apparatus would be used for about the same time, giving a cost of $0.08 per hour per student. The kymograph is, as we all know, in constant demand; in a busy laboratory where research is going on it will probably be used not less than 2 hours per day for 35 weeks of the academical year, or over 400 hours for research. For lectures on time, memory, motion, etc., it will be used for a total of, say, 5 hours per year for 40 persons, or 200 hours more. This makes 600 hours per year for $17.50 or about $0.03 per hour. Thus the kymograph is ten times as profitable as the harmonical, and nearly three times as profitable as the color apparatus.

This method can be applied to every piece of apparatus, every tool, every piece of furniture, etc., yielding a definite answer in each case. The trouble, of course, lies in estimating beforehand the amount of use to which an instrument will be put. The success with which this is done is what is known in the commercial world as "business sagacity," "business tact and experience," etc.

Perhaps in this connection it may be desirable to state the results of experience at the Yale laboratory with a few of the more important parts of the equipment. One of the most profitable parts is the lantern equipment, representing an investment of about $400, and used on about 68 occasions per year for periods of 10 to 30 minutes for classes of 60 to 130 students. Fully as profitable is the lathe equipment ($100) with motor ($150), shafting, belting, grindstone and the various accessories. The circular saw is also of creditable efficiency. The extensive equipment of small tools and supplies of all sorts is of special value where apparatus is

made. The total workshop equipment is worth about $400 exclusive of the constantly replenished supplies; it is in use eight hours a day by the mechanic and approximately four hours a day by workers in the laboratory. When we consider that much of our best research work and many of our demonstrations would be impossible without the aid of the workshop, we are forced to conclude that this equipment is very profitable in spite of the large expense for wages and materials. Moreover, in a new science where new pieces of apparatus are required for every important advance, the cheapest way of getting such apparatus is by making it in the laboratory. In regard to apparatus such high returns for investments are not to be expected; our most profitable pieces have been the recording drum for hand or motor, 100 v. d. electric fork, spark coil, multiple key, chronoscope, kymograph, piston recorder, etc. Apparatus used for research should be judged by the value of the results obtained.

In business there are losses as well as profits; in managing a laboratory we cannot always make the most judicious selections. Nevertheless, by applying these principles of laboratory economy we can make the money invested bring much larger returns than by spending it hap-hazard. The difference between an economically managed business and a loosely managed one is the difference between success and failure. Laboratory economy does not, after all, differ fundamentally from business economy. High grade efficiency cannot be attained anywhere unless the man at the head knows his business down to the minutest details. The best laboratory is not the one on which most money has been expended, but is the one that yields the largest net result in scientific research and in instruction for each dollar expended.

Economy in investigation.

In a large laboratory with many investigations going on, it becomes a very difficult task to adjust matters economically. Each investigator thinks only of his own needs and is generally oblivious to the fact that when a piece of apparatus is monopolized by him it is lost to every one else. He is likely, for example, to set up the kymograph in such a way that no one else can use it, whereas with just the same labor it might be arranged for every one. He should be taught to choose and arrange his apparatus so as to produce a minimum disturbance. Suppose that he wishes to produce a click for a warning-signal and that there are two sounders and only one relay in the apparatus case. The relay is generally the easiest to adjust; by taking it, however, he would get no better sound and would render it impossible for any else to use relay currents. The investigator should learn to use the cheapest materials at hand. If

for example, he wishes to conduct a telephone-current, he should not follow his first impulse to use very large, silk-covered, expensive flexible cords made for incandescent lamps, but should content himself with common office wire.

Still more important is it for the student to learn how to plan and execute his researches. The ignorant method of piling up indiscriminate measurements in large quantities leads to nothing. The problem for investigation should be first definitely stated. As soon as the work is sufficiently advanced to involve measurements, the investigator should make clear to himself whether the work is to consist in a determination of a single quantity or in a full investigation. In the former case he must, before making his final measurements, examine and consider all possible sources of error, and should carefully estimate and adjust them so as to obtain the required degree of accuracy. The instructions for this work and for carrying out the measurements are to be found in WEINSTEIN'S Physikalische Maassbestimmungen, Berlin 1886, Volume I., to page 282, omitting certain parts that refer to full investigations. In the case of a full investigation, where the object is the determination of a function expressing the dependence of one quantity on other quantities, the work becomes much more complicated. The student should be familiar with the whole first volume of WEINSTEIN. This work by WEINSTEIN is rather voluminous and it contains no psychological examples; it is to be hoped that a smaller work on the subject will be written specially for the use of psychologists, economists and biologists.

Any attempt to carry out serious investigations without a knowledge of the principles involved results in a wasteful piling up of figures without any adequate return in the way of laws or facts established. Such an extravagant procedure has become impossible in sciences like physics; it must soon become so in experimental psychology.

The proper understanding of methods of measurement and investigation requires a familiarity with at least the elements of analytical geometry and calculus. The student can get along with what is contained in NERNST-SCHOENFLIES'S Einführung in die mathematische Behandlung der Naturwissenschaften, or, in regard to calculus, with FISHER'S Infinitesimal Calculus, although a more extended study like that involved in working through such familiar books as STEGEMANN-KIEPERT'S Differential- und Integralrechnung or SCHLOEMILCH'S Compendium der Analysis with the examples given by FUHRMANN, Naturwissenschaftliche Anwendung der Differential- und Integralrechnung, is highly desirable. Without at least an elementary acquaintance with calculus and a familiar working knowledge of the science of measurements higher scientific work will remain unintelligible and inaccessible to the student of experimental psychology.

NOTES.

In reply to requests for the colors of the glasses employed in the color-sight tester described in these Studies, 1895 III 103, the following complete list is given: (1) dark gray; (2) ground glass; (3) medium gray; (*A*) red; (*B*) ground glass; (*C*) blue; (*D*) green; (*E*) green; (*F*) brownish yellow; (*G*) ground glass; (*H*) gray glass; (*I*) green; (*J*) bluish green; (*K*) red; (*L*) violet. This arrangement is made for reasons that will become evident to the users of the tester. When the test is being made, the subject should be made to promptly call off the colors in the order 1, 2, 3. The dichromats of the first-class (SEEBECK), or the so-called green-blind, make mistakes on light green and dark red; those of the second class, or the red blind, make mistakes on the light red and dark green. Both classes almost invariably call the violet "blue." Plain dark grays will be frequently called green by the first class, and red by the second. These results hold good for the color-weak of both classes, provided the instrument is used in a properly moderate light, for example, not in sunlight.

In order to advance psychological work at this period when good special instruments are hardly obtainable, the Director of the Yale Laboratory offers to furnish to experimental psychologists—as far as may be practicable—blue-prints of the working-drawings of any of the special Yale pieces. A nominal charge of ten cents each will be made to cover the cost of paper and mailing. The instruments can be made from these drawings by any laboratory possessing a mechanic. These drawings will not be supplied to apparatus-makers or other persons than psychologists except in special cases.

The following changes and corrections are to be made in the *Elementary course in psychological measurements* published in these Studies, 1897 IV.

Page 89, in line 9 from bottom read "for about two or three hours each."

Page 102, the formulas in the middle of page should read

$$A = \frac{A_r + A_l}{2},\ B = \frac{B_r + B_l}{2},\ \text{etc.}$$

Page 122, in lines 3 and 7 from top, and in line marked *D*, read "Ex. IX." instead of Ex. VI.

Page 122, lines 8 and 9 from top should read: "The $4A$ current is sent through the break contact of the key and the spark coil in the way explained in Ex. IX; the condenser is connected around the key as usual."

Page 122, in the line marked G insert 7^{σ} after the word "sounder."

Page 123, in the last line of the specimen record the decimal point has been omitted in two cases; they should read —4.6 and — 8.7 instead of — 46 and — 87 respectively.

Page 129, the last lines of paragraph p should read "Place the second contact at 80°, then at 120° and then at 160°; draw zero-lines as before."

Page 138, in line 8 from the bottom change ½ meter to "1 meter."

Page 139, in line 21 change $\frac{1}{2}m$ and $\frac{1}{2}n$ to "$1/2m$" and "$1/2n$."

Page 139, in line 16 change $2m$ and $2n$ to "$1/2m$" and "$1/2n$."

STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

EDWARD W. SCRIPTURE, PH.D.
Director of the Psychological Laboratory.

1898
VOL. VI.

YALE UNIVERSITY
NEW HAVEN, CONN.

THE NEW ERA PRINTING COMPANY, LANCASTER, PA.

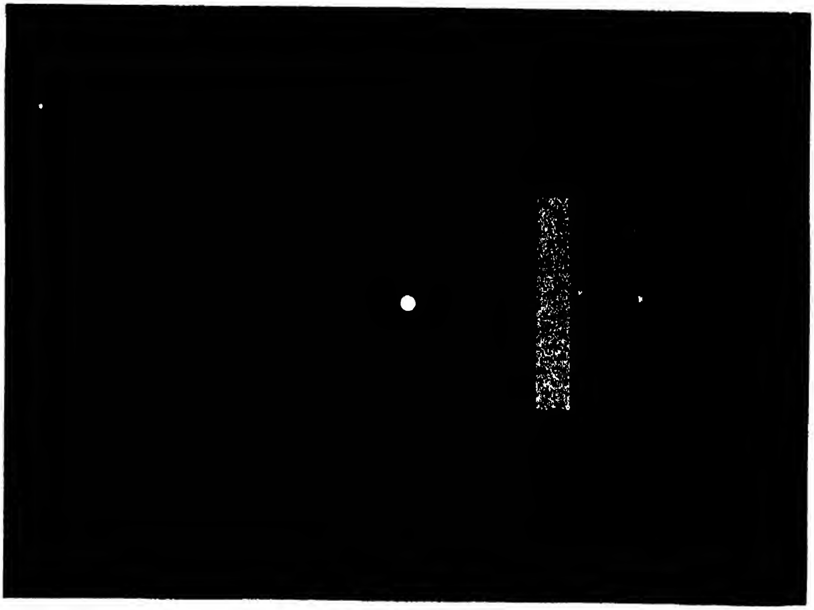
CONTENTS.

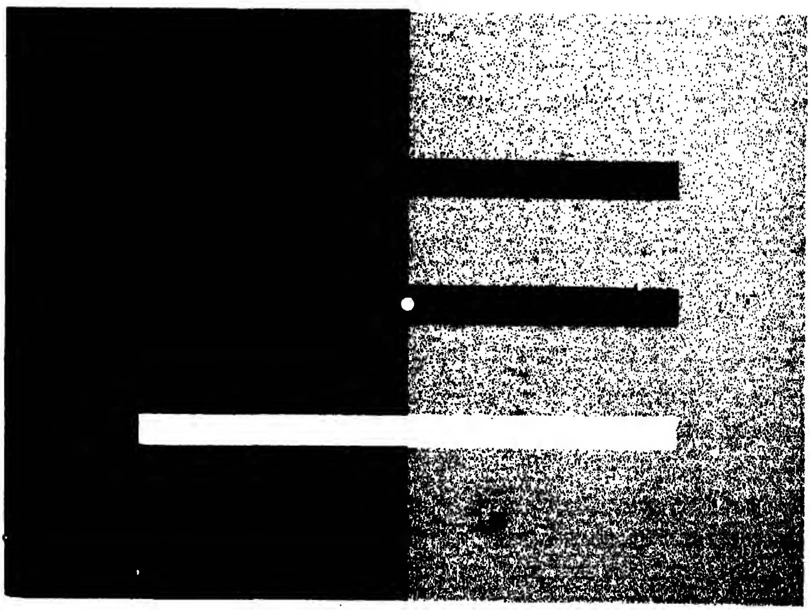
A color illusion, by Prof. George Trumbull Ladd, ... 1

Researches in cross-education, by Walter W. Davis, . . . 6

Researches in practice and habit, by W. Smythe Johnson, . . 51

Notes, ... 104





A COLOR ILLUSION,

BY

GEORGE TRUMBULL LADD.

Some time ago my attention was called by Dr. GEORGE T. STEVENS, of New York City, to a color illusion which, in spite of its suggestiveness, has not, so far as I am aware, been hitherto discussed or even noticed.

In FICK's Lehrbuch der Augenheilkunde, p. 50, Leipzig, 1897, there is a colored diagram called an "example of STILLING's charts" for the purpose of testing color blindness. It consists of a pale-green background, in size 36^{mm} by 44^{mm}, which is divided into squares of 1.8^{mm} by lines of white 0.4^{mm} in width; on this background a red letter E 21^{mm} by 34^{mm} is constructed out of similar red squares.

It was noticed that, when this figure was observed for a few seconds with a fixed gaze, some or all of the red squares disappeared and were replaced by green squares like those of the background. Practice had the usual result of facilitating the speed and completeness of this illusion; it soon enabled most of those on whom the experiment was tried to get the result in a more or less startling way. I will only add that with me the illusion is invariably connected with a conscious change in fixation of attention, and—if I may be allowed the expression—the internal motor adjustment; and the red squares always turn dark and become a blackish-green for an instant before they disappear and are replaced by the green of the color o the background.

The first suggestion for an explanation of this interesting illusion would connect it with the relations of the retinal images of the two eyes. But such an explanation is at once negatived by the fact that the illusion is obtained equally well, or even better, with one eye. It is thus not an affair peculiar to binocular vision. Neither is it a phenomenon of blurring due to minute rapid movements of the eyes, or to relaxation of the muscle of accommodation. For, although in certain experiments to be described subsequently, the color of the background does frequently seem to throw a film of its own color over the letter, square or strip, which it surrounds, in the original figure as taken from FICK there is no blurring of the red squares, and in all the other experiments the white

lines and outlines of the squares and the color of the background remain quite distinct and sharp.

Further experiments have been devised to investigate this illusion. Although no final conclusion has been as yet established, enough has been done to show that the phenomenon is more complicated than was at first supposed. A brief description of these experiments will now follow.

In the first place, within certain limits, not as yet fixed, changes in the size of the usual objects with which the experiment is conducted do not essentially alter the result. For example, if the diagram in FICK be looked at through a concave lens the diminished letter E will behave in the same way; although with me it is more difficult to get the illusion, because, as I think, both of the increased clearness of the images and also of the increased tension of the attention and the eyes' adjustment. On the other hand, as all our experiments show, much larger objects, when viewed from a sufficient distance behave in the same way. The exact limits of size of the retinal images within which this phenomenon is possible have not been calculated, but considerable variation in size is known not to be incompatible with its occurrence.

If, now, in order to test the importance of the white lines which divide, in the FICK diagram, both background (or "sulstituting" color) and letter (or "disappearing" color) into squares, we have a strip composed of red squares divided by white lines against an undivided background of lighter or darker green, we get the following result: the red turns dark green or black; the white lines remain; and the squares of red become patches of blackish color separated by white outlines on an unchanged color-background. If the background be changed from green to violet or blue the results are substantially the same. But the same change in color takes place with a solid red letter on a background of various colors; and with a strip of red or orange, not limited in any way by white outlines, on variously colored backgrounds. Indeed, in a number of instances, as I shall soon say, the background substitutes its color and the illusion takes place almost or quite equally well without either being itself divided or having the disappearing color divided into squares. The illusion, then, is not dependent upon the lines that divide the diagram into squares, although such a division seems to have some influence on the speed and completeness of the result.

A series of experiments was then devised by Dr. Scripture to test the effects of varying the color of the background, or substituting color, while retaining the same red as the disappearing color, without, however, dividing either background or strip into squares.

A complete set of the MILTON BRADLEY colored papers was obtained;

in the following experiments the colors will be designated according to the system adopted by the manufacturers. Sheets 20^{cm} by 30^{cm} were cut out for backgrounds; small strips 1^{cm} by 20^{cm} were cut ready to lay on the backgrounds. In making the experiment a background of a desired color was fastened to a board by tacks and a strip was fixed on it by a pin; the whole was then observed at a distance of about 3^{m}. It afterwards appeared that the most convenient method of preparing these figures was to place several strips of different colors on a given background; such a figure with two strips and a white fixation point is shown in the colored plate.

The results with Dr. Scripture and myself differed somewhat; but the interesting thing about these differences seems to be that they remained fairly constant of the same order. That is to say, where I got the illusion of substitution without great difficulty, my colleague got it more readily; and when I got it not at all, he could obtain it only with increased difficulty, or in certain cases (at least on the earlier trials) not at all. This suggests that the detailed differences were due to a constant factor of difference in the two observers.

The results obtained by viewing a standard red strip at a convenient distance, and with the proper fixation of the eye, while the color of the background is changed, may be divided into two general classes. With certain backgrounds the illusion of disappearance and substitution takes place with no great difficulty but with surprising ease and suddenness, after a little practice has given the requisite knowledge of how to experiment. With certain other backgrounds the substitution takes place only with increased difficulty or not at all, with me as a rule not at all. To the first class of backgrounds belong the two greens used, namely, standard green and bluish-green, violet, blue and black. To the second class belong the backgrounds, yellow, orange, light gray, white, light blue, light green and a light reddish violet. That is to say, this standard red strip, if viewed with a proper amount and kind of fixation on a background of two kinds of green, or of dark violet, dark blue, or black, will itself darken, disappear, and be placed for a longer or shorter interval by the color of the background. But the same strip, if placed upon a background of yellow, orange, gray, white, or light blue, light green, light violet, will maintain its place, although growing darker; or if it disappears at all, does so with great difficulty and only for an instant. As I have already said, I could not myself get the illusion in these cases at all.

Suppose now that a strip of orange be viewed upon the same backgrounds as in the foregoing experiments, then somewhat similar phenom-

ena result, but with interesting differences. With the backgrounds of green, dark violet, dark blue or black I get the illusion; but only by persistent trying, and for a briefest instant of time. On the backgrounds of yellow, gray, white, light blue, light green, violet of tint No. 1 Dr. Scripture obtains the illusion only with great difficulty, if at all, and then for a brief instant of time. But on the other class of backgrounds the illusion is rather more easily obtained by him with the orange than with the red strip. With me, however, on both classes of backgrounds, the red strip and the orange strip behave in markedly different ways. Whereas, in the case of the red strip the color darkens—and this whether substitution ultimately takes place or not, and whatever the color of the background—in the case of the orange strip, the color grows lighter, the color of the background seems to encroach from both sides on the color of the strip, until (where—as in all cases of the second class of backgrounds—the illusion does not become perfect) the orange strip becomes a narrow line of sunlight on a unicolored background.

If now the orange and the red strips be hung not far apart on the same background of color, each of them behaves, for both observers, in the same way as when viewed apart on a similarly colored background. And if orange and red strips be placed side by side, with one half over a background of the first class (see the colored plate) and the other half over a background of the second class, each half of each strip will seem to follow, for each observer, the course similar to that already described in the separate experiments.

This color phenomenon is probably a general one. It was observed by most persons at a meeting of the American Psychological Association at which these figures were exhibited. It was quite evident at the first trial to a girl fourteen years of age who was asked to observe the strips. I am not yet able to satisfy myself as to the most probable explanation of this somewhat startling color illusion. In the well-known case of a dark spot on a colored background Blix[1] has suggested that with long fixation the retina becomes less sensitive on the parts on which the colored rays fall, and more sensitive on the part corresponding to the dark spot, and that this latter portion, being sensitive to the colored light arriving after general dispersion in the eye, finally gives as intense a color sensation as the other portion. Any one, however, who has seen the red stripe suddenly turn to a bright green knows that the intensity of the green is far beyond any light that might arrive through dispersion.

Dr. Scripture has suggested that the fatigue of the eye for the color of

[1] Blix, *Ueber gleichfarbige Induktion*, Skand. Arch. f. Physiol., 1893 V 13 (reviewed in Zt. Psych. Phys. Sinn., 1894 VII 411).

the disappearing strip or letter creates a temporary blind-spot or succession of blind-spots, which is then filled in by the color of the background as the permanent blind-spot of the eye is constantly filled in by the surrounding colors in all our normal vision. This suggestion explains some of the phenomena which I have been describing. It explains the dependence of the result on the character of the fixation, the customary preliminary darkening of the disappearing color, and the character of the substitute color, when substitution takes place. But I am not able to reconcile this suggestion with the fact that the illusion takes place only with such great difficulty, or not at all, when the disappearing color is dark and the background is light. Would not one expect rather the opposite result, namely, that the darker background would more speedily and completely fill in the eye fatigued by the lighter color. Moreover, I cannot see why the orange strip under the principle of fatigue, should grow brighter and of a lighter shade, as it certainly does for my eye; and, in fine, why in the case of the second class of backgrounds, I am quite unable to fatigue my eye for either orange or red so as to obtain the illusion by substitution of the color of these backgrounds.

Finally, as far as I can determine in my own case, and by questioning several others with whom the experiment has been tried, the illusion is somehow dependent upon the rhythm of attention, and, in a limited way, it is under the control of will exerted through some obscure modification of the point and manner of regard. But whatever the *prima facie* explanations may be, the illusion seems to me unusually interesting and complicated, and in its suggestiveness quite worthy of further investigation.

RESEARCHES IN CROSS-EDUCATION,

BY

WALTER W. DAVIS.

I. HISTORICAL.

The term "cross-education" has been used[1] to express the fact that the effects of practice on one side of the body are transferred to the unpracticed side. The fact seems to have been first recorded by H. F. WEBER. In a communication to FECHNER,[2] he reports an observation made on his son. The boy had been taught to write entirely in one system of penmanship, a system that employs a free-arm muscular movement. At the age of thirteen he was able to write reversely with the left hand —in so-called mirror-writing—without having practiced such writing in the least, although the letters were not so regularly made as those made with the right hand. The reverse writing, when viewed in a mirror, or when looked at through the paper as it was held to the light, appeared very similar to the boy's ordinary hand-writing. Hence WEBER concluded that by the training of the right hand in certain methods of penmanship the left hand is also trained, unconsciously, to perform symmetrical movements. He noticed also that others trained by different methods, or by several methods, failed in the test.

FECHNER reached a similar conclusion from an experience of his own. In the course of a series of observations in which he wrote the figure *9* many times, left-handed, he noticed that when he took the pen in his right hand, he would unconsciously write the figure reversed with a movement that was symmetrical to that made with the left hand. A certain method of writing had become so "impressed upon his mind" that it became natural to write reversely with the hand not used.

[1] SCRIPTURE, SMITH AND BROWN, *On the education of muscular control and power*, Stud. Yale Psych. Lab., 1894 II 115.

[2] FECHNER, *Beobachtungen, welche zu beweisen scheinen, dass durch die Uebung der Glieder der einen Seite die der andern zugleich mitgeübt werden*, Ber. d. kgl.-säch. Ges. d. Wiss., math.-phys. Cl., 1758 X 70.

The experiments of VOLKMANN,[1] on the influence of practice upon the power for perceiving small distances, have a bearing on cross-education. By touching a WEBER'S compass to the skin on various parts of the body, he obtained records of the ability of the several members to distinguish the two points of the compass at the smallest perceptible spread. For example, the left arm on the dorsal side could distinguish the two points at 10.5 Parisian lines (23.6^{mm}); the right arm at 11.5 lines (26.4^{mm}). At the end of the practice which was continued for several weeks with the left arm alone, the records were: left, 5 lines (11.2^{mm}), right, 7 lines (15.7^{mm}). While the acuteness of sense on the left side was increased through local practice, it was also increased on the right side in portions symmetrical with the parts practiced.

To determine whether other than symmetrical parts are thus trained, VOLKMANN tested the points of the fingers of both hands and also the left arm. By practice of one of the fingers of the left hand, he found an increase in ability in all the fingers, but none in the arm. That is, in the education of certain parts, those parts symmetrical and closely related are educated also.

More recently SCRIPTURE[2] has made some experiments in muscular control and muscular power which prove quite definitely that practice of one arm in steadiness and strength reacts on the other arm as well. DR. W. G. ANDERSON, Associate Director of the Yale Gymnasium, experimenting with the spring dynamometer, has reached practically the same conclusion. BRYAN,[3] in testing the tapping ability of children of different ages, concludes that the right hand does not outgrow the left, but that, at certain ages, the left even gains on the right.

The following experiments were carried on, during the academical year of 1898–99, for the purpose of establishing more definitely the fact of cross-education or transference of practice and, if possible, of finding the causes of such transference.

II. RAPIDITY OF VOLUNTARY EFFORT.

The present investigation was begun by experiments in the rapidity of tapping on a telegraph key. The movement in tapping involved only a small amount of muscular strength. The weight of the finger was sufficient

[1] VOLKMANN, *Ueber den Einfluss der Uebung auf das Erkennen raümlicher Distanzen*, Ber. d. kgl.-sächs. Ges. d. Wiss., math.-phys. Cl., 1858 X 38.

[2] SCRIPTURE, SMITH AND BROWN, *On the education of muscular control and power*, Stud. Yale Psych. Lab., 1884 II 114.

[3] BRYAN, *On the development of voluntary motor ability*, Amer. Jour. Psych., 1892–93 V 201.

to press down the button of the telegraph key, so that the test was one of motor ability with the factor of muscular power seemingly almost eliminated.

Apparatus.

The tapping was done by the subject in a quiet room, while the results were recorded in an adjoining room. The number of taps in a given

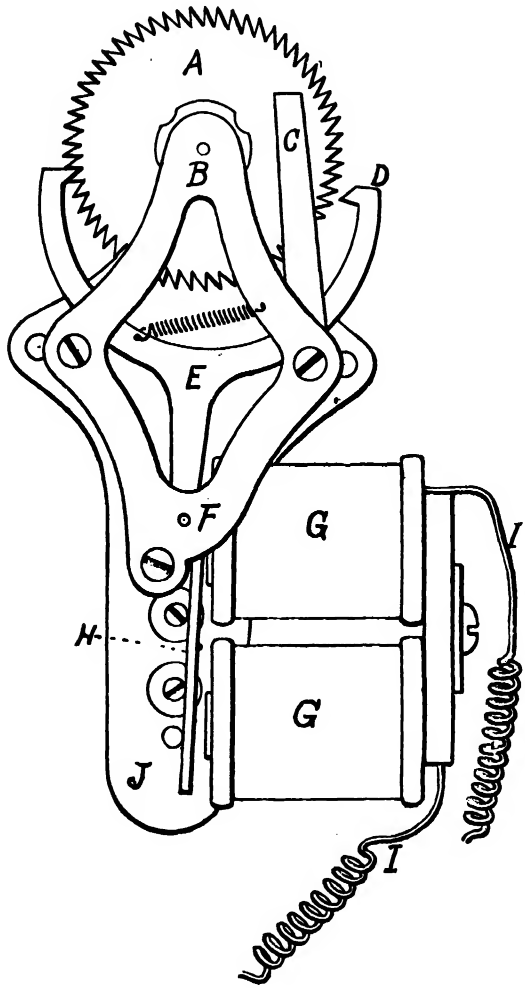


Fig. 1.

time was recorded on a tap counter which was connected by electric wires with the key. It consisted of clock-work with hour and minute

hands. At the back (Fig. 1) there is a toothed wheel worked by an electro-magnet (G) and an armature (H). When a current passes through the magnet, the armature is attracted; when it ceases, the armature is drawn back again by a wire spring. The upper end of the lever (E), of which the armature is a part, divides into two arms (D), each of which, one on each side, may press successively into a toothed wheel (A) in such a way that at each make and break of the electric current the wheel is driven forward one cog. This movement arises from the shape of the ends of the arms and of the teeth of the escapement wheel. A friction spring (C) hinders any backward movement. This counter has been in use in the laboratory for several years. Its special merit lies in the fact that it will count the most rapid taps that a person can possibly make. The number of taps is recorded on the face of the counter. A key within easy reach makes it possible, while the subject is tapping in the quiet room, for the observer to let the current pass or not, as he chooses.

In the quiet room, three keys were so arranged that the subject could, without changing his general position, tap with either hand or either foot. One key served for the hands and was clamped to a board which was held in the lap. With the subject seated in readiness for tapping, the board was supported by the arms of the chair and the elbows rested easily on the board, the hand just reaching the key. The tapping was done with the index finger, the subject being instructed to keep the other fingers, the wrist and the forearm on the board. This position allowed a rapid, easy, isolated movement without the use of clamps or weights that might distract the attention.

For the feet a board was so constructed that two keys, four inches apart, could be set into the top, their ends touching at one edge and leaving the buttons only a little more than flush with the surface of the board. The tapping was restricted to the great toe, all movement being confined as much as possible to this member. The chair remained always in the same position and the board could be moved on the floor, forward or backward, to suit the convenience of each subject. When a certain position was adopted for any subject, the conditions remained constant for him through the entire series of experiments.

For the purpose of communication the two rooms were connected by a system of signals, by which the observer could direct the subject to start or stop. In all cases the number of taps in five seconds was recorded. With all in readiness, the observer, with watch at hand, signaled the subject to start. At three seconds after the start the switch was closed and the counter began to record. After five seconds the switch key was opened and the subject signaled to stop.

Method of experiment.

In this series of experiments, the results of which are shown in Table I., tests of tapping-ability were made on six subjects. Of these, A. is an instructor, and B., C., D. and F. are students in Yale University. E. is the steward of the Psychological Laboratory. Initial tests were taken of the right and left index fingers, and of the right and left great toes. Then for periods varying from ten to twenty days the *right great toe* was practiced daily, or nearly so. At the end of the practice period final records were taken of each member. The initial and final tests were taken in the following order: for subject A. *LF RF LH RH RH LH RF LF;* for B. *LF RF LH RH LF RF LH RH;* for C., D., E. and F., *LF RF RH LH LH RH RF LF* (*RH*—right hand, *LH*—left hand, *RF*—right foot, *LF*—left foot). In this way the liability to differences due to fatigue was guarded against. In each initial and each final test, averages were taken of the two records made by each member. A specimen record is inserted, the first one taken.

Subject A.—Initial test.

	LF	*RF*	*LH*	*RH*	*RH*	*LH*	*RF*	*LF.*
	13	17	24	26	28	24	19	11
Average.	12	18	24	27				

The initial and final tests were taken at the same time of the day, and, as nearly as possible, under similar conditions. Lombard[1] and Dresslar[2] have pointed out the differences in voluntary strength and rapidity due to varying conditions. An effort was made to equalize them for all records.

Results.

The results obtained are shown in Table I., which gives the numbers of taps in five seconds for each member at both initial and final tests, also the gain in the number of taps, and the gain as a percentage of the initial number.

The effects of practice are not uniform. Not only is there a great difference between the gains made by different individuals, but also between the gains of different members of the same individual. Still, certain fundamental results are clearly shown. In the first four subjects, whose initial and final records are given in Table I., a marked inctease in the rapidity of tapping ability has been developed; and this increase mani-

[1] Lombard, *Some of the influences which affect the power of voluntary muscular contraction*, Jour. Physiol., 1892 XIII 2.

[2] Dresslar, *Influences affecting the rate of voluntary movement*, Amer. Jour. Psych., 1892 IV 514.

fests itself not only in the right foot, which alone was practiced, but in all the other members as well. However, the greatest average gain is

TABLE I.

Number of taps in 5 seconds.

Subject.		Initial test.	Final test.	Gain.	Relative g
A.	*LF*	12	17	5	0.41
	RF	18	20	2	0.11
	LH	24	28	4	0.17
	RH	27	32	5	0.18
B.	*LF*	15	17	2	0.13
	RF	14	21	7	0.50
	LH	22	36	14	0.64
	RH	22	34	12	0.54
C.	*LF*	20	26	6	0.30
	RF	21	28	7	0.33
	LH	32	36	4	0.13
	RH	37	42	5	0.14
D.	*LF*	17	25	8	0.47
	RF	20	28	8	0.40
	LH	26	34	8	0.31
	RH	27	29	2	0.07
E.	*LF*	16	22	6	0.38
	RF	21	20	—1	—0.04
	LH	22	24	2	0.09
	RH	24	22	—2	—0.08
F.	*LF*	16	17	1	0.06
	RF	21	19	—2	—0.09
	LH	30	24	—6	—0.20
	RH	33	28	—5	—0.15
Average.	*LF*	16	21	5	0.31
	RF	19	23	4	0.21
	LH	26	30	4	0.15
	RH	28	31	3	0.08
Average: A, B, C, D.	*LF*	16	21	5	0.31
	RF	18	24	6	0.33
	LH	26	34	8	0.31
	RH	28	34	6	0.21

made by the member practiced. The gain by *RF* is slightly greater than that of *LF* and *LH*, and considerably greater than that of *RH*.

In neither E. nor F. is there a gain in the member practiced, but rather a slight loss. In only one member not practiced, *LF* of subject E., is there a marked gain, while in *RF*, *LH*, and *RH* of subject F., are there marked losses.

In the second average the records of subjects E. and F. were not used, the object being to give the comparative average results of practiced and unpracticed members in subjects who made a gain in the member practiced.

From certain observations taken during experimentation, as well as from remarks and suggestions made by the subjects themselves, these facts appear to be satisfactorily explained. Subject E. is a colored man, the steward of the laboratory, thirty-four years of age, of a phlegmatic temperament, steady-going rather than quick and active. The two facts of age and temperament are suggested as the reason for his not responding more quickly to practice. In the gymnasium it is illustrated every day that after a certain age the boy loses in a great degree the ability to learn new tricks. Moreover boys differ individually in the ability to learn. This difference is not due wholly to lack of strength but to temperament, will-power and previous training as well. Possibly, with longer practice, good results might have been secured from E. Subject A. was of the same age as E., but the practice in his case was continued twenty days and in the case of E. only ten days; the two subjects differ much in temperament, A. being nervous rather than sluggish.

TABLE II.

Influence of age; number of taps in 5 seconds.

Subjects.	Age.	Average gain of the four members.	Relative gain.	Days of practice.	Relative gain per day.
A.	34	4	0.20	20	0.01
B.	29	9	0.44	14	0.031
C.	25	6	0.22	10	0.022
D.	27	6	0.27	10	0.027

Age seems to play an important part in the results obtained from practice. Table II. gives the ages of the subjects in connection with the average gain they make for both hands and both feet, the length of practice, and the gain per cent. for each day of practice. To obtain a fair increase in the rapidity of tapping, the practice of A., who is 34 years old, had to be continued for 20 days, and then the gain is less than that of B., C. and D., where the practice was continued for 14, 10 and 10 days respectively, and the ages were 29, 25 and 27 years.

An interesting principle was exemplified in the failure of F. to make a gain in rapidity by practice. F. is an academical senior, aged 23, the youngest of the six subjects, and of a nervous motor temperament. He is a trained gymnast of more than moderate skill. One would judge that conditions here were favorable for a rapid gain in tapping ability. Some of his remarks, however, after the daily practice, throw light on the difficulty. "If I try to hurry too much my foot stops almost altogether." "At times I am obliged to put forth my entire will power in order to tap at all." "I feel fatigued all over"; that is, fatigue did not confine itself

to the muscles involved but was general. It would seem that, in this subject, temperament and training had combined to form conditions unfavorable to rapidity of tapping. Naturally of a quick, nervous, active disposition, his training as a gymnast had emphasized these qualities. The proper execution of difficult gymnastic tricks requires a great exertion of strength and also its quick concentration at a particular moment in time. The subject had become so accustomed to sending down strong impulses to action to large muscles, that it was difficult for him to send proper impulses for the action of small muscles like those of the fingers and toes. Subject F.'s great store of energy is illustrated in his long practice records. It seemed impossible to tire him out. In one experiment he made 900 taps without a slowing in rapidity of tap-time. He concentrated more attention at one point than was favorable for rapidity of movement. So great an amount of nervous energy was sent to this point that the delicate muscles could not properly dispose of it for action, and the result was a slowing of movement.

On the whole, the subjects were a group not favorable for rapid improvement from practice. Of an average age of 28 years, they had gotten beyond the point in physical development where either muscular or nervous changes would take place rapidly. With younger subjects larger and more rapid gains might reasonably be expected. Nevertheless, the principle of cross-education appears most plainly and decisively.

Since obtaining the above results, I have received a letter from Herr Oscar Raif, Professor of Music in the Berlin Hochschule, who has done some experimenting similar to mine. I quote a portion of his letter, inserting the number of beats per minute in [ ].

"In the spring of '98, I made an experiment with twenty of my pupils. I began by taking the average speed of each hand with the metronome. The average of the right hand was ♩ = 116 (= four times 116 in the minute) [464 beats] and for the left hand, 112 [448 beats]. I gave them exercises for the right hand only (finger exercises, scales and broken accords) to develop rapidity. After one week the average of the right hand was 120 [480], after two weeks, 126 [504], three weeks, 132 [528], etc. After two months the right hand yielded 176 [704]. Then I had them try the left hand which averaged 152 [608], whereas in November the average was only 112 [448]. In two months' time, absolutely without practice, the left hand had risen from 112 [448] to 152 [608]. A few of my pupils had some difficulty in playing the scales in parallel motion, but were able to play them in contrary motion.

"The tenor of my work is that in piano playing the chief requirement is *not* that each single finger should move rapidly but that each movement

should come at exactly the right time, and we do not work only to get limber fingers but more than that to get perfect control over each finger. The source of what in German is called 'Fingerfertigkeit' is the center of our nervous system, the brain."

Further explanation of the application of these principles in musical training is promised in a work on "Fingerfertigkeit" by Professor RAIF. The fact that Herr RAIF's pupils could play the scales in *contrary* more easily than in *parallel* motion, deserves notice as coinciding with the observations made by WEBER and FECHNER in regard to mirror-writing.

Table III. gives the relative rapidities of hands and feet, both before and after practice, and their average percentage of gain. The ratio of tapping ability is smaller at the end of the practice than at the beginning; the relative gain of the feet being 5.5% greater than that of the hands.

TABLE III.

Comparative rapidity of hands and feet.

		Initial.	Final.	Relative gain.
A.	Feet.	15	18	0.24
	Hands.	25	30	0.16
	Ratio.	1:1.7	1:1.6	
B.	Feet.	14	18	0.31
	Hands.	22	35	0.58
	Ratio.	1:1.5	1:1.8	
C.	Feet.	20	27	0.32
	Hands.	34	38	0.12
	Ratio.	1:1.6	1:1.4	
D.	Feet.	19	26	0.39
	Hands.	27	31	0.17
	Ratio.	1:1.4	1:1.2	
Total Average	Feet.	$17\frac{1}{8}$	$22\frac{1}{2}$	0.31
	Hands.	$27\frac{1}{8}$	$33\frac{1}{2}$	0.26
	Ratio.	1:1.6	1:1.5	

In the case of D. two facts are noticeable from an examination of the table: (1) lower ratios between feet and hands both before and after practice; and (2) a greater percentage of gain in the tapping ability of the feet in comparison with the other subjects. During D.'s practice it was observed that he had an almost independent use of his great toe. He could flex or extend it, with very little accompanying movement of the remaining toes. D. is a Japanese student and while in Japan wears the ordinary clog shoe which allows free movement of the great toe. A heavy string extending up for an inch from the middle forward part of the sole divides into two parts which pass back, one on either side of the foot. When the shoe is adjusted to the foot, the string before its division

is grasped between the first and second toes, and the shoe is held in place by this means. The sock also is made in the form of a mitten, the great toe being in a separate compartment from the others. The sock is the only covering of the foot above and to the sides. Hence two prime conditions for development are here present, exercise and room for growth. These facts probably account for the smaller ratio between the rapidity of toes and fingers in the case of D. They also emphasize the fact already pointed out [1] that the feet of Americans are losing, as a result of tight, ill-formed shoes, those powers natural to them. The reason why the feet in the case of D. should make a greater percentage of gain than in the other subjects, although not so clear, is probably due to the same cause.

Influences affecting the rapidity of tapping.

In the daily practice the right great toe tapped until fatigued. There was, however, no attempt to reduce the muscles to a state of extreme fatigue, since the increase in tapping ability at the final test was the prime object. The records were made in a manner similar to those at the initial and final tests. The signal to start was given; after three seconds the switch was closed and a record was obtained for five seconds. The result was noted while the subject continued to tap without stopping, and after ten seconds another record was taken. The subject and the observer continued in this manner, the record being taken at every third period of five seconds, until the subject stopped tapping.

A study of these records and of the remarks dropped by the subjects led to several important observations.

(*a*) There are perceptible variations in the rapidity of the tap-time on any given occasion of practice. One subject says: "At times it is very easy to tap rapidly; then it becomes difficult; then easy again." Another subject noticed what he termed "waves," that is, short periods of rapidity followed by a slowing of the tap-time. These waves of rhythm have been noticed by NOYES [2] in the knee-jerk; and also by LOMBARD [3] in his work with the ergograph. They seem to be wholly beyond the control of the will.

This phenomenon is shown in Fig. 2 which gives the curve for a record by A., the twelfth in his series of practice records. X indicates the serial number of the record, and Y the number of taps in five sec-

---

[1] ELLIS, *The human foot*, Wood's Medical and Surgical Monographs, April 1890.

[2] NOYES, *On certain peculiarities of the knee-jerk in sleep*, Amer. Jour. Psych., 1892 IV 343.

[3] LOMBARD, *Alterations in the strength which occur during fatiguing voluntary muscular work*, Jour. Physiol., 1893 XIV 98.

onds. The continual fluctuation between gain and loss in energy is quite striking.

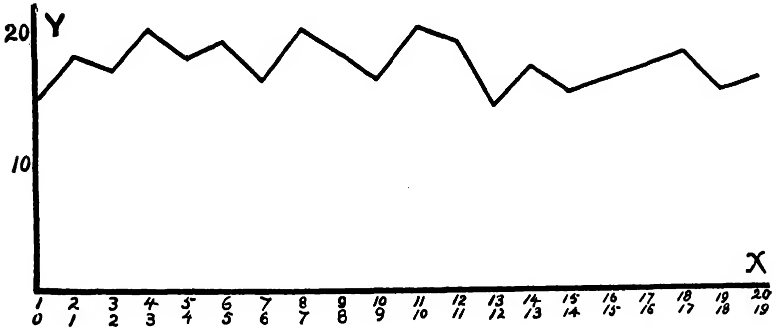


FIG. 2.

X, upper line, serial number of record.
X, lower line, successive steps in the subject's practice.
Y, number of taps in 5 seconds.

(*b*) States of feeling do not appear to have much effect on the rapidity of tapping. Often the subject would remark that he did not feel like making a good record and when the record was taken it would prove excellent. The reverse is true also. One of the subjects remarked before a certain practice that he had partaken of a fine punch the night before, had had a good night's rest, felt in excellent spirits and expected to make an unusual record. After the experiment the result was compared with the previous ones and the taps were found to be slower than any in the preceding three days. There were many other similar, though less notable, instances of the deceptiveness of the subject's judgment of his own condition.

(*c*) On the other hand, the physical condition of the hand or foot as judged by sensations had a noticeable effect on the tapping ability. On one very cold morning the subjects, without exception, made poor records; most of them had complained of cold feet, due to the exposure of the bare foot in the cool air.

TABLE IV.

Effect of exercise.

Day,	1	2	3	4	5	6	7	8	9
Record during first 5 seconds,	16	15	14	19	17	20	23	17	23
Record during second 5 seconds,	24	20	18	24	24	28	28	22	22

(*d*) Tapping could be done faster and more easily after a few taps had been made, or as C. expressed it, after the muscles had been "warmed

up." Table IV. gives the first two records of each of the practice experiments for D.

The results show that either the muscular or the nervous system or both do not do their best work at the start.

(*e*) Pain and fatigue, when they were noticed at all as the result of continued tapping, were generally located in or near the muscles employed in the movement. Rarely indeed was fatigue spoken of as being general.

(*f*) The length of time during which the subject kept up his practice was a good indication of his store of energy. There was a tendency to longer practice-records on Mondays and on any day preceded by a day of rest from tapping. Apparently the muscular or nervous energy expended in the performance of one day's task was not fully restored in one day, the tissues requiring a day's rest to regain their normal capacity for work.

(*g*) Tapping became much easier after a few days of practice; the apparent reason was that it did not require so much attention. This points to the probability that lower, automatic centers were being developed for the foot in the specific movement of tapping, leaving less responsibility for the act upon the higher centers of consciousness and attention.

(*h*) Subject A. experienced sensations of pain in his unused left great toe similar to those in the right which was being fatigued by the tapping. He has noticed the same fact in connection with "writer's cramp" which has troubled him at various times; the numb sensation of the right middle finger is sometimes transferred to the left middle finger for periods varying from a few minutes to hours. He does not use the left hand for writing.

The loss of the effects of practice.

To determine if the effects of practice are retained for a long period of time, A.'s record was obtained six weeks after his final test. Table V. shows the results compared with his initial and final tests.

TABLE V.

Effect of a long interval.

Subject.		Initial.	Final.	After six weeks.
A	*LF*	12	17	18
	RF	18	19	16
	LH	24	28	24
	RH	27	32	30
Average		20	24	22

There was a general loss after the interval. This loss was more marked in the hands than in the feet; the average loss for the hands was 3; for the feet, 1.

III. Strength of voluntary effort.

It has already been proved that the development in the strength of one arm is accompanied by an increase in the exertion that can be put forth by the other.[1] Is this increased power due to increased nervous activity or to increased muscular tissue?

The following experiments were undertaken primarily to determine if an increase in the size of one arm would result from the exercise of the other in muscular strength and endurance. From previous experiments, referred to in Sec. I., an increase of strength was looked for in both arms. It was not believed that a perceptible increase in the girth of the left arm would result after so short a period of training.

Method of experimenting.

The experiments were made in the following way: six subjects were chosen, definite girth measurements of both arms were taken, and the number of times ascertained that each arm could raise a weight of 2¼ kilos. (5 pounds).

At the initial test the subject's clothing was removed from the upper part of his body. His weight was then taken and his strength of forearm, or grip, measured by the usual oval spring dynamometer. The following measurements were then made by Dr. J. W. Seaver, Associate Director of the Yale Gymnasium: right and left upper arm both flexed and extended; right and left forearm with and without the hand clenched. These measurements were taken at the largest circumferences of the arm above and below the elbow. The weight (a 2½ kilo. dumbbell) was then given to the subject, who was instructed to lift it from a position where the arm hangs extended downward and the weight is supported from the shoulder, to one where the arm is flexed and the weight close to the shoulder. In this movement the elbow remains stationary. Hence, to accomplish this act, the biceps is employed almost wholly, though the muscles of the forearm are also used to a lesser extent in gripping the dumbbell. This gripping was intensified toward the end of the test, when the subject became fatigued.

A metronome was not used, but each subject was allowed to fall into his own rhythm of movement, which varied according to the length of the arm

[1] Scripture, Smith and Brown, *On the education of muscular control and power*, Stud. Yale Psych. Lab., 1894 II 114.

and the man's temperament. The only instruction he received was not to allow the weight to stop at its highest and lowest points. At the final test it was observed that the rate of motion was unconsciously faster than at the initial test. This was probably due to an increase of energy stored up during the practice.

Fatigue was not carried to an extreme, because such a test would put the arm and the physical system in general into such a condition that good results could not be expected from the succeeding short practice. When the right arm was fatigued, a rest of five minutes was given, after which the left arm went through the same exercise.

The subject then entered upon a practice extending from two to four weeks; this consisted in simple flexions of the *right arm* with the weight. The subjects were instructed not to tire the arm but to exercise it frequently and lightly, rather than heavily and at long intervals.

At the final test the same data were obtained in the same way and under the same conditions as at the initial test. Additional data were also obtained, to be spoken of later. The arms were examined by pressure to detect any changes in condition that might have been occasioned by practice.

Characteristics of the subjects.

As the results indicated that the age, physical condition and mode of life of each subject should be taken into consideration, the following data were collected:

G. Age, 28. Health, fair. Temperament, nervous. Muscles, soft, undeveloped. Exercise, light gymnasium.

H. Age, 26. Health, good. Temperament, phlegmatic. Muscles, soft. Exercise, light.

B. Age, 29. Health, good. Temperament, nervous. Muscles, soft. Exercise, none. Left-handed in most actions.

I. Age 26. Health, good. Temperament, nervous. Muscles, firm, well developed Exercise, regular, in gymnasium.

J. Age, 26. Health, excellent. Temperament, motor. Muscles, well developed, but soft. Exercise, none.

K. Age, 24. Health, not good, over-worked. Temperament, nervous. Muscles, soft and very poorly developed.

The subjects were all in a muscular condition favorable to increase in girth measurements, but their average age of 26 years was probably unfavorable for such an increase. They were all members of the Graduate School of Yale University. Those who were taking any physical exercise, were instructed to continue it throughout the practice.

Increase in the dimensions of the arms.

Table VI. shows the girth measurements taken at the initial and final tests and the increase due to the practice. The subject's age and weight are given as a means of control. The measurements are in millimeters.

TABLE VI.

Increase in arm girths.

Subject. Age.		Biceps. Right. C.	Biceps. Right. R.	Biceps. Left. C.	Biceps. Left. R.	Forearm. Right. C.	Forearm. Right. R.	Forearm. Left. C.	Forearm. Left. R.	Wt. in kilos.
G. 28	Initial.	283	228	274	226	259	255	256	246	56.3
	Final.	288	232	269	226	260	255	254	246	56.8
	Gain.	5	4	—5	0	1	0	—2	0	0.5
H. 26	Initial.	270	238	265	237	252	247	250	248	66.6
	Final.	283	246	271	241	265	257	260	252	67.2
	Gain.	13	8	6	4	13	10	10	4	0.6
B. 29	Initial.	287	249	285	252	246	237	260	252	75.7
	Final.	293	255	296	256	259	249	265	257	75.1
	Gain.	6	6	11	4	13	12	5	5	—0.6
I. 26	Initial.	230	275	317	275	283	275	281	272	71.6
	Final.	238	281	320	277	283	277	277	270	71.6
	Gain.	8	6	3	2	0	2	—4	—2	0
J. 26	Initial.	310	265	302	260	270	262	265	255	69.3
	Final.	312	264	302	262	273	265	266	260	69.3
	Gain.	2	—1	0	2	3	3	1	5	0
K. 24	Initial.	276	235	260	220	252	240	243	232	56.1
	Final.	280	239	262	230	251	240	246	235	56.1
	Gain.	4	4	2	10	—1	0	3	3	0
	Av. gain.	6.3	4.5	2.8	3.6	4.8	4.1	2.1	2.5	0.1

C = Contracted. R = Relaxed.

The results showed that the practice had effected an increase in the girth measurements. For example, G.'s initial measurement for the right biceps contracted was 283, relaxed it was 228. His final measurements for the right biceps were, contracted 288, relaxed 232. The gain in the biceps contracted was 5^{mm} and relaxed 4^{mm}, as the result of his two weeks' practice.

There was no marked gain in the weight of any of the subjects that might account for the increase in girth. The average gain was 0.1 kilo, which is insignificant, while B., who lost weight, made as large gains in girth as any.

The gains in girth measurements were greatest in this order; right biceps, right forearm, left biceps, left forearm. The right arm gained by direct practice but though the right increased twice as much as the

left, still the left made marked gains, and there seems to be no doubt that there had been a transference of the effects of practice on the one side, to the unpracticed other side. Among the girths of right biceps there is only one loss recorded. This is for J., who made only small gains in all of his girths. For left biceps there is one loss recorded, that of G. This, as well as other irregularities in the case of G., to be mentioned later, may be explained from the fact that he misunderstood the instructions given, and during his practice period, stopped his usual exercise. For the right forearm one small loss is recorded, and for left forearm three losses.

The results, though exhibiting variations and exceptions, show very clearly that exercise producing a gain in girth of one arm causes a similar though smaller gain in the other.

Symmetrical development.

Here, it would seem, is a provision by nature to prevent a one-sided development. If the right side of the body received all the benefit of its excess of exercise over the left, it would tend to outgrow it in much greater proportion than is actually the case.

There is really very little difference between the sizes of the right and left arms. This is especially true if the measurements are taken with the muscles relaxed. There is a greater difference when contracted, as if a stronger stimulus to action could be sent to the right than to the left. As a proof of this point the measurements of one hundred Yale Freshmen were averaged, and the following results obtained.

Right biceps, contracted,	296.04^{mm}
Left " "	282.57
Difference,	13.47
Right biceps, relaxed,	248.95
Left " "	242.41
Difference,	6.54

The difference in girth between the right and left biceps, contracted, is 13.47, but when relaxed, the difference is only 6.54. If we subtract the girths with muscles relaxed, from the girths with muscles contracted, we find a difference in the right arm of 47.09, in the left of 40.16. These last figures may represent the contracting power of the muscles, and if so, the ability of the left arm is about 85% that of the right.

This difference in the extent to which the muscles are contracted is shown very clearly in my own case. This measurements are: right biceps contracted 330^{mm}, relaxed 275^{mm}; left biceps contracted 317^{mm}, re-

laxed 275^{mm}. The measurements are the same for the muscles in the relaxed condition, but when contracted the difference is 13^{mm}, in favor of the right. There is a difference also, in the contours and in the lengths of the two biceps. The right is shorter and more clearly defined, an indication that it has greater power of contraction.

Increase in endurance.

In the initial test the subject raised the weight until forced to stop on account of fatigue or pain, though the test was not carried to the very extreme of endurance. The number of flexions made is given in Table VII.

TABLE VII.

Number of flections made.

		R	*L*			*R*	*L*
G.	Initial.	180	100	I.	Initial.	250	223
	Final.	1000	300		Final.	1000	250
	Gain.	820	200		Gain.	750	27
H.	Initial.	100	80	J.	Initial.	200	75
	Final.	1050	110		Final.	600	300
	Gain.	950	30		Gain.	400	225
B.	Initial.	100	125	K.	Initial.	136	93
	Final.	1000	200		Final.	860	607
	Gain.	900	75		Gain.	724	514

This table also gives the number of flexions made in the final test. The weight which in the initial test was heavy enough to reduce either arm to a condition of fatigue after a few minutes' work, was not able to so effect the right in the final test, though soon fatiguing the left arm. The practice had so inured the right arm to fatigue that with the given weight and time of flexion the work could be kept up almost indefinitely. At one thousand flexions the fatigue was scarcely noticeable, hence the test was not continued.

It is clear that the right arm had developed remarkably in endurance. K. who is slight and not of rugged health, at the end of the final test for right arm, said his arm was not tired in the slightest degree, although he was apparently almost overcome by the general effects of fatigue. The left arm also had gained in endurance though not to so great an extent. The average number of flexions for the left arm at the initial test was 119, at the final test, 297, a gain of 178 or 150%. We conclude that while the left had gained very materially, both in size and endurance, from the practice of the right, there was still a lack of that fineness of condition which seems dependent upon actual exercise of the muscle itself.

There is an entire lack of correspondence between the increases in

girth and in power of endurance. In fact the six subjects illustrate two types, differing widely in these respects. G., J. and K. belong to a type showing very little gain in girth, but exhibiting a marked increase in endurance of the left arm. H., B. and I. make large gains in girth for both right and left arms, but little increase in the endurance of the left arm.

TABLE VIII.

Comparison between girth measurements and power of endurance.

		Girth gains.		Flexion gains.
		Right biceps.	Left biceps.	Left arm.
	G.	5	—5	200
Type A.	J.	2	0	225
	K.	4	2	514
	Average	3⅔	—1	313
	H.	13	6	30
	B.	6	11	75
Type B.	I.	8	3	27
	Average	9	6⅔	44

In Type A (Table VIII.) there is an average gain of 3⅔^{mm} in the girth of the right arm and a loss of 1^{mm}, in that of left; while the flexions for left arm increase by 313. In Type B the girth gains are large, but the gain in flexions is only 44. The two types are, then, very clearly defined and separated from each other; while the similarity of the individual results in each type is quite close. For example, G.'s gain in right biceps is less than any in Type B. The same comparison can be made successfully with any individual whatsoever in either type. The facts noted are therefore worthy of the highest consideration.

Why such results are produced is not clear. Though it has often been noticed that a larger muscle is not always capable of greater strength and effectiveness than a smaller one, still one would think that there ought to be a correspondence in the same individual at different times between the size of his muscles and their endurance, and that if a marked gain in size occurred for any reason there should be also a marked gain in endurance. In Type A the nutrition has effected such a change in the muscle cells that they have gained endurance. In Type B the tissue that has been added may be fat, or some other constituent that has not been worked into the life of the muscle cell.

Increase in strength.

The strength of forearm was taken at both initial and final tests by means of an ordinary oval spring hand-dynamometer. The subject was given two trials with each hand and the highest mark made by each hand

recorded. No practice was given the subject in gripping the dynamometer, the object being to determine if there was any increase in strength of the right forearm due to general improvement in the nutrition of the arm. This being found, it remained to be determined whether there was a similar increase in the strength of the left. It has already been pointed out that as fatigue came on there was a certain amount of clenching of the hand in holding the dumbbell. One would reason that the muscles of the forearm would be developed and so an increase of strength would result. This is found to be true. Table IX. shows that this increase is

TABLE IX.

Increase in Strength.

		Pressure of dynam. in kilos. *R*	Pressure of dynam. in kilos. *L*			Pressure of dynam. in kilos. *R*	Pressure of dynam. in kilos. *L*
G.	Initial.	41.8	43.6	I.	Initial.	54.1	49.0
	Final.	45.4	40.8		Final.	56.8	53.1
	Gain.	3.6	— 2.8		Gain.	2.7	4.1
H.	Initial.	46.5	35.4	J.	Initial.	38.6	38.2
	Final.	54.1	48.9		Gain.	44.5	41.8
	Gain.	7.6	13.5		Final.	5.9	3.6
B.	Initial.	38.6	39.5	K.	Initial.	46.5	41.8
	Final.	54.5	50.0		Gain.	44.2	45.4
	Gain.	15.9	10.5		Final.	— 2.3	3.6
					Average gain	5.56	5.41

transferred to the left side. The average gain of the right arm is 5.56 kilos., of the left 5.41 kilos. or nearly as much. This corresponds closely with the figures obtained by Dr. ANDERSON, where the dynamometric pressure itself was practiced. He found, with practice of the right hand alone, a gain in the right of 11.7 pounds, in the left of 13.2, the gain in the left being the greater.

There were two failures to make gains in strength of grip. G., who, as we have before pointed out, neglected to follow instructions exactly, made no increase in the left forearm; and K., who showed no increase in girth of right arm, failed there also to increase in strength.

If we compare the gain in girth of forearm (hand clenched) with the gain in pressure, we find a close correspondence. The six subjects again fall into two types, not so clearly defined, however, as those in Table VIII. In Type C (Table X.) are placed H., B. and J., who show the largest gains in girth and also in the dynamometric pressure. Though there are some partial variations from the type, the averages prove the point very conclusively. The average gains in girth and in pressure in Type C are all large; in Type D they are all small, zero or minus.

A test of endurance differs materially from one of strength. The former requires a succession of small impulses for action, extending over a long period of time; the latter, a strong impulse for action for only a

TABLE X.

Comparison between girth measurements and dynamometric pressure.

		Girth gains. Forearm.		Dynamometer gains.	
		R	*L*	*R*	*L*
	H.	13	10	7.6	13.5
Type C.	B.	13	5	15.9	10.5
	J.	3	1	5.9	3.6
	Average	$9\frac{2}{3}$	$5\frac{1}{3}$	9.8	9.2
	G.	1	—2	3.6	—2.8
Type D.	I.	0	—4	2.7	4.1
	K.	—1	3	—2.3	3.6
	Average	0	—1	1.3	1.6

moment of time. The two tests are very unlike, and may require the development of entirely different factors.

The effect of practice on the ability to resist fatigue and pain.

Some important facts were noted from the observations made in respect to fatigue, pain and soreness, due to exercise; and also in respect to the condition of the muscle, before and after practice. At the initial test the fatigue was local for both right and left arms, and was limited to pain in the attachments of the biceps muscles at the shoulder and elbow. The biceps itself did not tire. No general feeling of fatigue was experienced. For a few days after the initial test the muscles and tendons of both arms were very sore, so that practice was quite materially interfered with. No marked difference could be detected, by sight or pressure, between the muscular condition of the right and left arms.

At the final test fatigue was more general. K. was completely "tired out." Subject I. "ached in knees and back," was very nervous and could not sleep the first night after the test; his arms felt numb with a tendency to "go to sleep." J. experienced a great thirst during the test. When local pain was felt at all it was generally in the tendons, as at the initial test. Very little soreness either in the right or left arm was experienced as a result of the final test. When the muscles were pressed with the finger a slight difference could be noted between the condition of the arms, the right biceps being the firmer. The facts above

noted do not hold for all of the subjects. The data could hardly be exhibited in a table, so they have been summed up in general.

We may draw several conclusions from the facts observed in regard to fatigue, pain and soreness. (1) Practice so inured the right arm to its work that in the final test general fatigue came on before local fatigue. This hardening process was transferred in a striking degree to the unused side. In a work of endurance the tendons seem to weaken before the muscles themselves. (2) By practice the right arm reached such a condition that the after-effects of local soreness from continued exertion were avoided. This was found equally true of the unused side. (3) As far as could be judged from the examination by pressing the muscles with the fingers and from the amount of work done by both arms, the right arm had attained a fineness of condition not shared in by the left.

The immediate effects of exercise on girth measurements.

In order to determine if the blood circulation on one side of the body varied with the exercise of the other side, measurements of the biceps, contracted and relaxed, were taken in the order *R*, *L*, before the exercise with the dumbbell; then in the order *R*, *L*, immediately after the test with the right arm; and finally in the order *L*, *R*, immediately after the test with the left arm. The measurements given in Table XI. do not show a corresponding variation in both members.

TABLE XI.

Girth measurements before and after exercise.

		Biceps. 1st Meas.		Biceps. 2d Meas.		Gain over 1st.		Biceps 3d Meas.		Gain over 1st.	
		R	*L*	*R*	*L*	*R*	*L*	*R*	*L*	*R*	*L*
G.	C	288	269	293	268	5	—1	293	277	5	8
	R	232	226	245	228	13	2	240	240	8	14
H.	C	283	271	298	268	15	—3	290	273	7	2
	R	246	241	256	239	10	—2	261	246	5	5
B.	C	293	296	307	300	14	4	296	303	3	7
	R	255	256	267	255	12	—1	260	270	5	14
I.	C	338	320	348	320	10	0	347	330	9	10
	R	281	277	294	276	13	—1	291	292	10	15
J.	C	312	302	318	304	6	2	314	315	2	13
	R	264	262	274	260	10	—2	265	270	1	8
K.	C	280	262	290	261	10	—1	277	270	—3	8
	R	239	230	252	231	13	1	243	238	4	8
					Average C	10	$\frac{1}{6}$		Average C	4⅓	8
					R	12	—3		R	5½	10⅔

C—Contracted. R—Relaxed.

In the second set of the measurements there was an average increase of

10 mm, or 3%, in the girth of the right biceps contracted, and of 12mm, or 5%, in the same relaxed. There was no increase in the left arm, but rather a decrease in size. The fact that the increase in right arm was greater in the relaxed condition, may be explained mechanically. The surplus of blood due to exercise was actually squeezed out of the muscle when it was rigidly contracted.

The increase in the size of the right arm is probably due to two effects of exercise: (1) the rush of blood to the muscle, shown by the distention of the superficial veins; and (2) the swelling of the muscle due to the production of heat and waste products. After K. had completed his test, a marked difference in temperature was noted between the biceps and the triceps. The fact was evident even to the sense of touch. The triceps felt cold in the comparison.

In the third set of measurements taken immediately after exercising the left arm, this member was found to have increased in girth, while the right, due to its quiescence, had already lost much of its former gain. In J. and K., who made the highest number of flexions with the left arm, the former increase made by the right is practically all lost.

Three facts would seem to show that the circulation of the side not exercised does not tend to vary in accordance with that of the side exercised: (1) the negative results in the left arm measurements taken after the exercise of the right; (2) the rapid decrease in girth of right arm, after its exercise, even though the left was then exercising; (3) the very manifest difference between the temperature of used and unused muscles in close proximity. The facts would indicate rather a variation of blood circulation in the opposite direction.

TABLE XII.

Amount of Practice.

	Total number of flexions.	Days of practice.	Av. flex's. per day.	Time of exercise.
G.	4900	12	408	Evening.
H.	5500	12	458	Evening.
B.	3100	12	258	Eve. and morn.
I.	3900	12	325	Eve. and morn.
J.	1050	18	115	Evening.
K.	6300	21	300	Morning.
Average	4125	14½	310	

Amount of practice.

For the practice experiments the instructions were not to tire or overwork the muscle but to exercise lightly and frequently. To show the

amount of practice done Table XII. was prepared from the subjects' notes. The duration of practice averaged $14\frac{1}{2}$ days. The average member of flexions in each day's practice varied from 115 to 458, the total average being 310. The time of exercise was either early morning or late evening. No correlation could be discovered between the length and time of practice and the girth gains, or the number of flexions and the girth gains. The two subjects, J. and K., who continued the practice over the longest period of days made the greatest gains at the final test in the number of flexions of the left arm. The average of the periods of practice was $19\frac{1}{2}$ days and their average gain in flexions, 369. For the other subjects the period of practice was 12 days and their gain in the left arm flexions only 83. This indicates that the transference of the effects of practice is not immediate, but occurs *after* the effects are noticeable on the side practiced.

TABLE XIII.

Comparison of results.

Subjects.	Conditions.			Results.							
	A	B	C	D		E		F		F	
				R	*L*	*R*	*L*	*R*	*L*	*R*	*L*
G.	28	12	408	5	—5	1	—2	820	200	3.6	—2.8
H.	26	12	458	13	6	13	10	950	30	7.6	13.5
B.	29	12	258	6	11	13	5	900	75	15.9	10.5
I.	26	12	325	8	3	0	—4	750	75	2.7	4.1
J.	26	18	115	2	0	3	1	400	225	5.9	3.6
K.	24	21	300	4	2	—1	3	724	514	—2.3	3.6
Average	$26\frac{1}{2}$	$14\frac{1}{2}$	310	$6\frac{1}{3}$	$2\frac{5}{6}$	$4\frac{5}{6}$	$2\frac{1}{6}$	757	178	5.56	5 41

A, subject's age.
B, practice period in days.
C, daily average number of flexions.
D, girth gain of biceps, contracted, in mm.
E, girth gains of forearm, contracted.
F, gains in number of flexions.
G, gains in dynamometric pressure, in kilos

Summary.

Table XIII. is intended to exhibit at a glance the important conditions of the dumbbell test and the results obtained in relation to the amount of practice. For example, H., 26 years of age, during a period of 12 days, by lifting a weight of $2\frac{1}{4}$ kilos. with the right arm 458 times each day, increased the girth of the right biceps by 13^{mm}; of left by 6^{mm}. The averages give the results which may be expected with the given conditions. The figures would vary undoubtedly with another set of subjects or should any of the important conditions be changed. The general condition to be emphasized is that by practice of one side of the body

in muscular power the other side shares in the gain in size, strength and endurance.

IV. Accuracy of voluntary effort.

Lunging at a target with a fencer's foil was chosen as a suitable exercise to educate the subjects in accuracy and coördination. It is a complicated movement, involving the simultaneous action of many muscles and muscle groups. These coördinated muscles are in some cases remote from one another. Accuracy in the movement may be cultivated to considerable fineness. Attention is essential to such accuracy.

The lunge used in this work was that taught by the French school of fencing. It is described as follows: (1) The subject—when lunging right-handed—at the command "Ready," assumes a position with the right side to the target, left foot parallel to the target, right foot in advance a short step and at right angles to the left, knees slightly bent, body perpendicular, left arm bent over the head, right arm with foil in hand supine, right elbow bent and foil pointed at the target. (2) At the command "Lunge," the following movements are executed simultaneously: the whole body is thrown forward toward the target but the trunk is still perpendicular; the right foot is advanced a long step, but with knee still bent; the left foot is kept in its place and the left leg extended; the left arm drops to the back; the right arm is extended.

In this way the foil is advanced at the target. This lunge was taught as rapidly as possible during the practice. It required about a week for the subjects to learn it with any degree of grace and precision. They were allowed to practice the movements, right-handed, in their rooms but no records of accuracy were made except at the regular practice hour, in the presence of the investigator. The practice continued in most cases for 10 days.

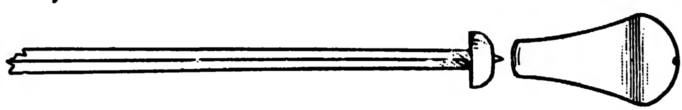


Fig. 3.

Apparatus.

A tack was soldered to the point of an ordinary fencer's foil. (Fig. 3.) The tack was filed to a sharp point of 3^{mm} length. A rubber cap was adjusted firmly over the end of the foil so that the point of the tack was hidden. When the foil was thrust against the target, the rubber was compressed so that the tack protruded enough to pierce the paper; the elasticity of the rubber, as the foil was drawn away, preventing any tear-

ing of the paper that otherwise would have been caused. The target (Fig. 4) was devised by Dr. Scripture. It was composed of two boards 60^{cm} square, and 2^{cm} and 1^{cm} thick. The lighter swung on the heavier one by a hinge at the top, thus forming a cover over it. A disc 47^{cm} in diameter was cut out of the thin board so that the center of the circle and the center of the boards coincided. At the ends of the perpendicular and horizontal diameters of the circle, four small nails with sharpened tops were inserted into the back board. Thin white wrapping paper was used for targets. To place a target the thin cover-board was raised, the paper was pressed down on the four nail points, the cover was lowered and firmly clamped. The whole target was

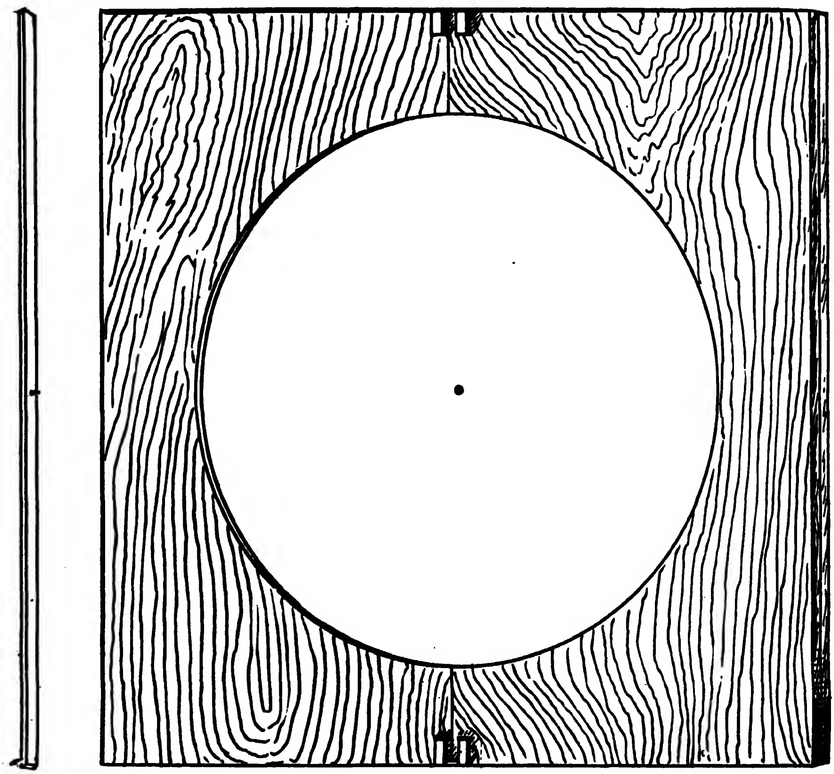


FIG. 4.

securely fastened against the wall at the height of $1\frac{1}{4}$ ^{m}. To center the target: a stick (Fig. 4) was fitted into two sockets on the cover. A nail with a sharp top was fixed in the middle of the stick, so that it

coincided with the center of the target. When the paper had been adjusted, the stick was placed in the sockets and by pressure on its upper surface the nail on its lower surface was driven through the paper into the center of the target. The stick was then removed, and a black pin 15^{mm} long, with a head $2\frac{1}{2}^{mm}$ in diameter, was inserted into the center to serve as a point to aim at. For each subject a distance from the target was chosen which seemed best adapted for his reach with the foil. This distance remained constant for him during the entire practice.

Characteristics of the subjects.

Four of the subjects were college freshmen, one a graduate student, and one a draughtsman.

Their individual characteristics may be indicated in the following manner:

I. Age, twenty-six. Graduate student. Temperament, nervous. Gymnast and athlete.

L. Age, seventeen. Temperament, nervous. Ambidextrous. General training in athletics.

M. Age, seventeen. Temperament, phlegmatic. No gymnastic training.

N. Age, twenty-two. Draughtsman. Temperament, motor. General training and especially in gymnastics. Ambidextrous.

O. Age, eighteen. Temperament, motor. No systematic bodily training.

R. Age, eighteen. Temperament, nervous. No special training except farm work.

The average age of $19\frac{2}{3}$ years should be favorable for increase in accuracy and coördination.

Method of experiment.

For the test the subject was allowed either to dress in a gymnasium suit or to wear his ordinary clothes. In the latter case, however, the coat, collar and tie were removed to allow of free movement in lunging. After the manner of dressing had been chosen, it remained the same for each subject throughout the tests and all the practice exercises.

None of the subjects had fenced previously to the experiments. When the subject came into the room for the first time, the lunging movement was described carefully, but briefly, and each was allowed two or three preparatory lunges—right-handed—before a record was taken. Then, at the command "Ready," he assumed the position, and at "Lunge," he thrust the foil at the target. The observer stood ready with his pencil

and marked the hole in the paper with a figure 1. The lunge was immediately repeated until a record of ten thrusts was obtained. The paper was then removed and a fresh one placed in the target. Any important observations were also noted down. After five minutes the experiment was repeated with the left hand.

The subject was practiced for ten days in thrusting with the *right hand.* Ten thrusts were allowed for each daily practice. At the end of the practice period, the initial test was repeated to compare gains in the right and left arms. Notes were also taken in respect to improvement in *form* of lunging. The tests were all taken about 3 P. M., and for each subject all the conditions were kept as nearly as possible the same.

Results.

The figures tabulated in Table XIV. are distances in mm. from the center of the target and are the averages for ten thrusts. Subjects L. and N.

TABLE XIV.

Increase in accuracy of lunging.

		Initial.		Final.		Gain.		Relative gain.	
	Age.	*R*	*L*	*R*	*L*	*R*	*L*	*R*	*L*
I.	26	21.8	46.0	16.8	34.5	5.0	11.5	0.22	0.25
L.	17	36.2	39.9	25.4	27.2	10.8	12.7	0.29	0.30
M.	17	49.8	72.8	25.3	44.4	24.5	28.4	0.49	0.39
N.	22	56.2	50.4	25.7	35.4	30.5	15.0	0.54	0.29
O.	18	80.2	86.5	25.5	32.7	54.7	53.8	0.63	0.62
R.	18	59.2	47.9	27.5	44.9	31.8	3.0	0.53	0.06
Average	19⅓	50.5	57.2	24.3	36.5	26.2	20.7	0.51	0.36

are practically ambidextrous, and in right and left initial tests they give average results not very widely separated. R. shows greater accuracy with the left than with the right although he is right-handed. In the final tests the right hand is more accurate in all cases. In three cases the left has made the greater gain in millimeters; in the other three, the right. Four subjects made greater relative gains with the right arm; two, with the left arm. The final averages show that the right arm is the more accurate. In the initial tests the average difference between right and left is 6.7^{mm}. In the final tests the difference is 12.2^{mm}. Hence the right has made the greater average gain, the difference being 5.5^{mm} in favor of the right.

Table XV. gives the probable error, expressed in millimeters and also in per cent. of subjects O. and I. The point to be noted especially is

that the probable error of the left arm has decreased as well as that of the right, though in each case the decrease is greater in the right.

TABLE XV.

Decrease of probable error.

		Probable error.		Probable error in %.				Probable error.		Probable error in %.	
		R	*L*	*R*	*L*			*R*	*L*	*R*	*L*
I.	In	12 46	12.02	15.5	13 9	O.	In.	31.2	4.92	14 3	16.9
	F.	2 86	3 62	11.2	11.		F.	1.84	3.74	10.9	10.8

Figs. 5 and 6 were constructed to illustrate the increase in accuracy

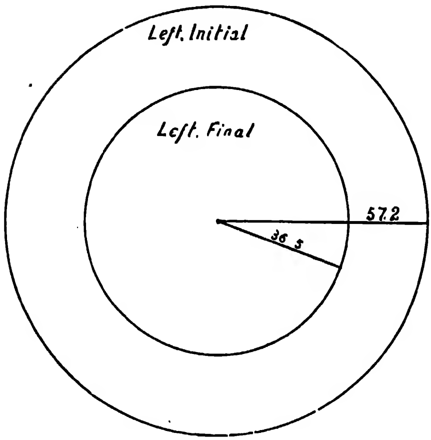


FIG. 5.

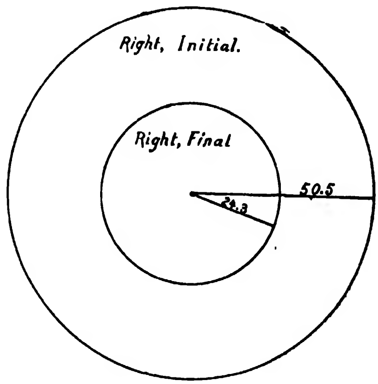


FIG 6.

due to practice in lunging. The cuts exhibit the facts given in Table XIV. The average distances from the center in the initial tests are indicated by the outer circles; the distances in the final tests by the inner ones.

The effect of previous training.

The fact that the left does not gain so much as the right is emphasized by the consideration that, since the right arm is, to begin with, more accurate than the left, there is less opportunity for it to make large gains. There is a point beyond which increase in accuracy is extremely slow. The largest gains were made by subject O. whose initial records were the most inaccurate of all. The smallest gains were made by subjects I. and L. whose initial records were the most accurate.

Subject I., who is a trained man physically through gymnastics and athletics, made the most accurate average initial record. He was able

after 15 days' practice to lower the record of the right arm by 5^{mm}, and that of the left arm by 11.5^{mm}. M. and O., who were very inaccurate at the initial test, had had no special bodily training; at this particular test of accuracy they made great improvement. The others did not exhibit striking characteristics in the records; they were men of average bodily training. An exception to the preceding statement is to be made in the case of R., in whose case the left hand was superior to the right in the initial test and made only a small gain.

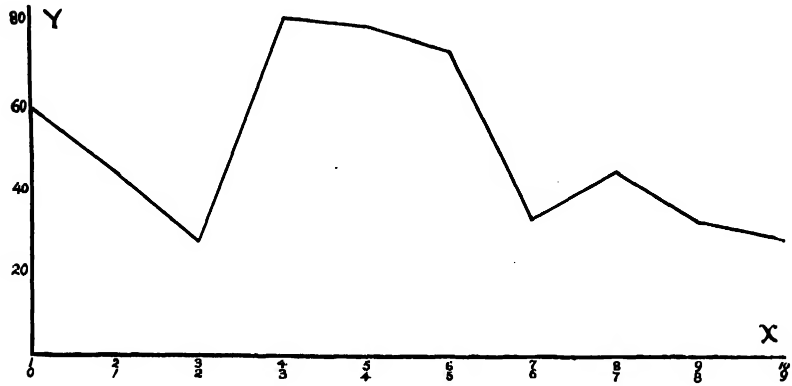


Fig. 7.

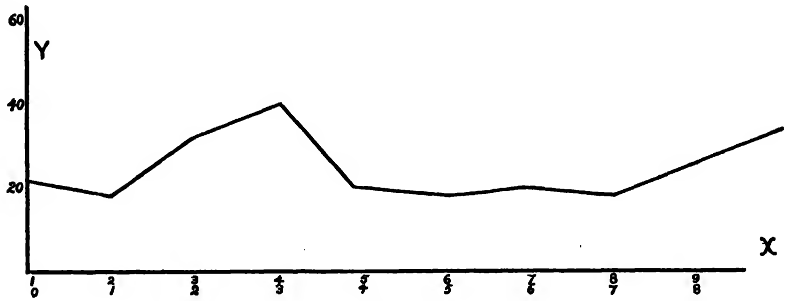


Fig. 8.

X, upper line, serial number of thrust.
X, lower line, successive steps in subject's practice.
Y, distance from center of target.

It is probable that accuracy, steadiness and coördination, when secured through any means of training, make their influence felt in any test that requires such qualities for its successful performance.

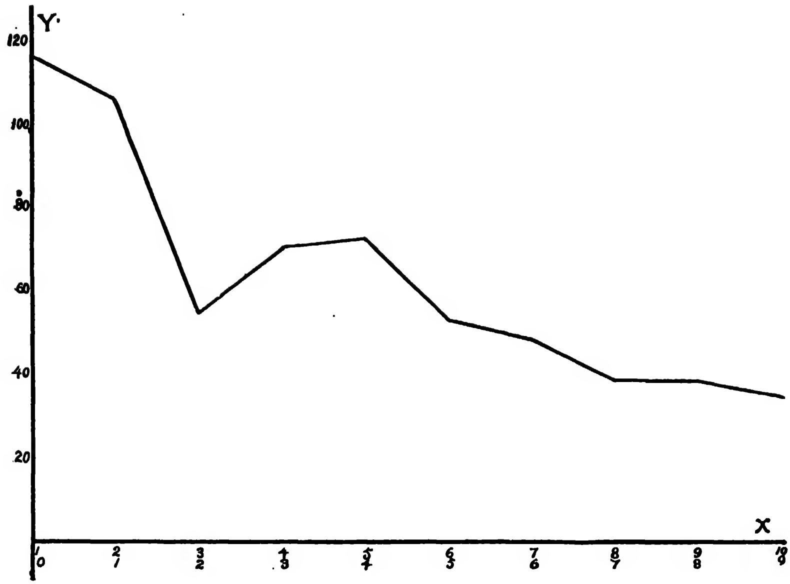


FIG. 9.

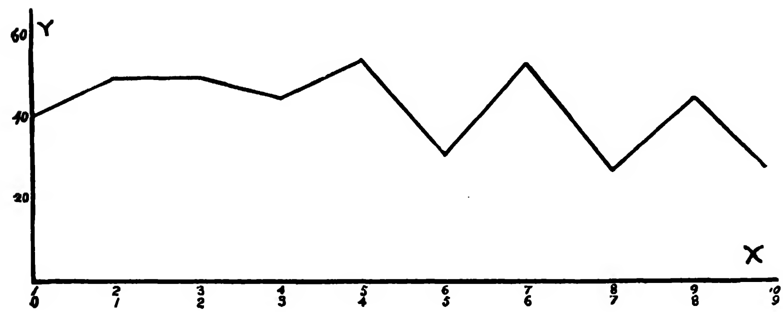


FIG. 10.

X, upper line, serial number of thrust.
X, lower line, successive steps in the subject's practice.
Y, distance from center of target.

Influence of practice on average accuracy.

The curves in Figs. 7, 8, 9, and 10 were made to illustrate the average accuracy of the successive thrusts in the initial and final tests. *X* indicates the number of the thrust in the series of ten, and *Y* the distance in millimeters from the center of the target. The initial and final records of subjects I., M., O. and R. were averaged to form the curves, the records of L. and N. having been cast out because these subjects were ambidextrous.

In Fig. 7 (initial test for the right hand lunge) the curve is irregular, reaching its greatest inaccuracy at the fourth thrust. At the final test for the right hand lunge (Fig. 8) the curve is more regular, but resembles the initial curve in still being most inaccurate at the fourth thrust. The curve for the initial test for the left hand lunge (Fig. 9) shows an increase in accuracy. The curve for the final test of the left hand lunge (Fig. 10) has been changed very materially by the practice of the right arm. The most important fact shown by the curves is that by the right arm's practice, both right and left have gained greater steadiness and uniformity. In the final tests each thrust is more nearly an average thrust.

Coördination of Movement.

Accuracy and coördination are closely connected. Accuracy depends in great part upon a delicate coördination of muscle groups and of motor centers. A few facts in respect to the subject's "form" during his performance of the lunges are here presented; the term "form" is used to mean the grace and precision with which the movement is executed.

When the foil was handed to the subject for the first time, he was instructed how to use it right-handed. After the short rest between right and left tests, he was asked to lunge left-handed, but no additional instructions were given. In most cases there was no confusion resulting from the change of side, though the form was not so good. At the final test the same points were observed, with the following results.

I. Movements with the left hand were executed correctly and in order, but a certain awkwardness was experienced.

L. (ambidextrous). Form almost perfect with the left hand.

M. Was not able during the practice to learn to dispose of the left hand properly. He always forgot to lower it when executing the lunge. This same trouble was experienced at the final test with both right and left hands. Otherwise the form was good, both right and left.

N. (ambidextrous). He remarked that it was easier to lunge left-handed than right-handed.

O. Form not so good left-handed as right-handed.

R. Not at ease left-handed. This shows in the records. Very little gain was made for the left arm.

In general, the movements necessary to the lunge are learned for the left side by practicing the right. Yet there is a perfection of grace and a fineness of coördination, that are attained only by the actual practice of the member under consideration.

Accuracy of skilled fencers.

The records of four experienced fencers, all of whom teach the art, were secured and are shown in Table XVI. Of these F.L., N.L. and G.M. teach with the foil in the right hand and have never fenced left-handed. W.A. though right-handed, teaches with the foil in the left hand. For

TABLE XVI.

A comparison of the accuracy of skilled and unskilled fencers.

		Average accuracy. *R*	*L*	Average of *R* and *L*.
	F.L.	36.3	63.3	49.8
	N.L.	42.6	58.9	50.7
	G.M.	40.5	51.6	46.0
Average		39.8	57.9	48.8
	W.A.	24.9	23.8	24.3
Six subjects of Table XIV.	Initial.	50.5	57.2	53.8
	Final.	24.3	36.5	30.4

comparison with these records, the table gives those of the six subjects of investigation. The experienced fencers who had taught right-handed were not able to equal in accuracy the six subjects who had been practiced for ten days; in fact they were not greatly better than those subjects at the initial test. W.A. is physically a perfectly trained man, and a teacher of gymnastics. These facts, together with his left-handed teaching, probably account for his accuracy. The inaccurate records of F.L., N.L. and G.M. may be explained in two ways. (1) In fencing, accuracy is not cultivated to any great extent,[1] but skill in parrying and the ability to get the thrust in at the right moment are considered the essential points. (2) Much of the accuracy of the six subjects is due to their practice under exactly the same circumstances. Possibly skilled fencers would be enabled to make unusually accurate records by a few days of similar practice.

Ratio of accuracy between right and left arms.

Table XVII. shows the ratios in accuracy between right and left arms. With one exception, the ratio has increased, as the result of the practice with the right arm. It is significant that the average final ratio of the six subjects is nearly the same as the average ratio of F.L., N.L. and G.M. For W.A. who fences with either the right or the left hand, the right and left sides are practically equally accurate.

---

[1] SCRIPTURE, *Tests of mental ability as exhibited in fencing*, Stud. Yale Psych. Lab. 1894 II 123.

TABLE XVII.

	Initial ratio.		Final ratio.	
	R	*L*	*R*	*L*
I.	1	2.11	1	2.05
L.	1	1.02	1	1.07
M.	1	1.46	1	1 75
N.	1	0.89	1	1.37
O.	1	1.08	1	1.28
R.	1	0.80	1	1 63
Average	1	1.13	1	1 50
F.L.			1	1.74
N.L.			1	1.38
G.M.			1	1.27
Average			1	1.45
W.A.			1	0.95

In the case of the skilled fencers, very little difference could be detected between the "form" of the right and the left sides. The fencers themselves were surprised to find it so easy to lunge left-handed. Not only was there no apparent awkwardness, but the new movement was executed with a considerable degree of precision and accuracy.

Fatigue.

The test was too short to allow the element of fatigue to enter to any extent. In many individual records, however, fatigue, or what produced similar effects, was present. Figs. 11, 12 and 13 are curves constructed from individual records. *X* is the number of the thrust in the series, *Y*, the distance of the thrust from the center of the target. The curve in Fig. 11, a practice record of subject L., shows a tendency to improvement in accuracy which is regular till the eighth thrust. After this, fatigue seems to come in and the accuracy decreases with the tenth. In Fig. 12 the curve of M. shows improvement till the fifth thrust, in which the center is struck. Fatigue appears earlier in this record and there is a general decrease in accuracy to the end of the record. In no case did the subjects complain of fatigue, but the observer could detect a wavering of the foil point toward the end of records like Figs. 11 and 12. Though the subject was not conscious of it, the fatigue was evident in the decrease of accuracy and in the unsteadiness of the foil.

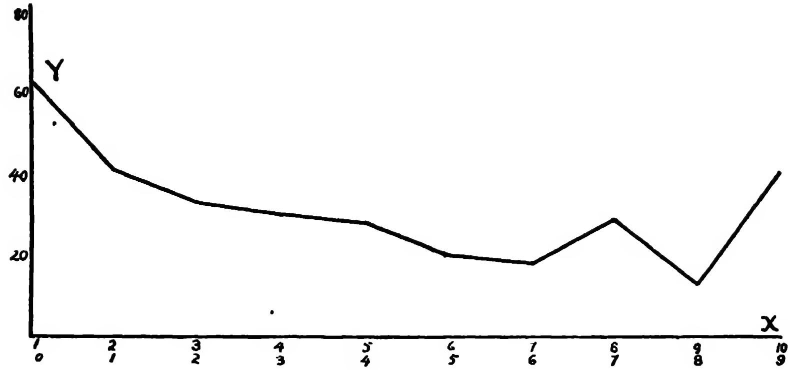


FIG. 11.

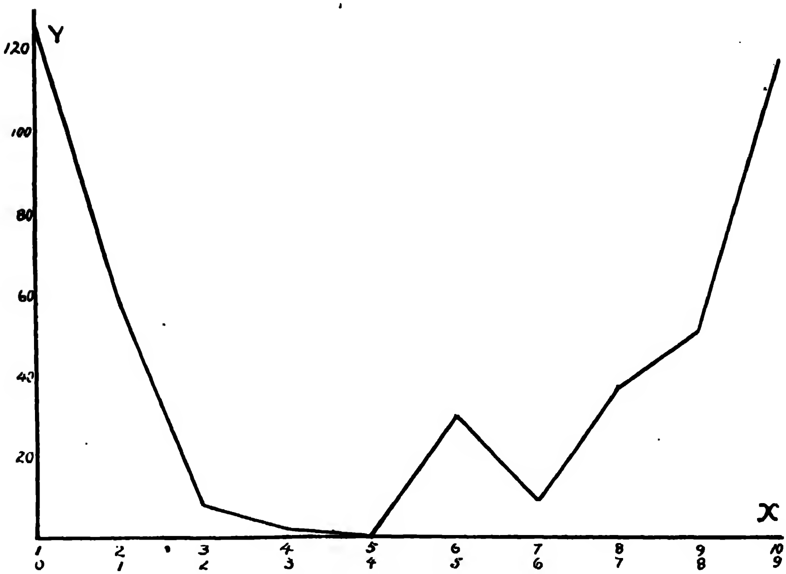


FIG. 12.

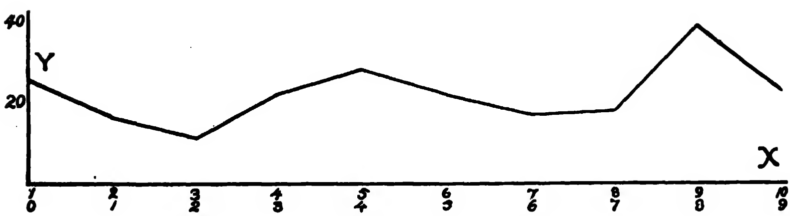


FIG. 13.

X, upper line, serial number of thrust.

X, lower line, successive steps in subject's practice.

Y, distance from center of target.

In N.'s fourth practice record, shown in Fig. 13, there is no great variation from the first thrust. After each thrust the subject's nervous equilibrium is reëstablished before the next thrust is made.

The form of any individual curve is dependent on three factors. (1) The condition of the nervous and muscular systems. The exercise of the nerves and muscles in lunging tends to improve their condition and hence accuracy is increased. If the system is in prime condition, due to other exercise just previous, this factor does not enter in largely. (2) Fatigue. This factor is more likely to affect the latter part of the curve. (3) Recovery. This factor is in opposition to (2). If the system is able to recover itself completely after each thrust the effects of (2) are counterbalanced. In Fig. 9 the average of left initial test, factor (1) is evident. The accuracy increases from the first to the last because the condition of the nervous and muscular systems is being improved by exercise. In Fig. 12, factor (1) is evident in the first part, factor (2) in the last part. In Fig. 13, factor (2) is counterbalanced by (3).

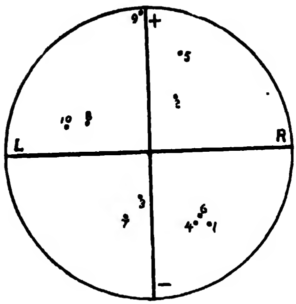


FIG. 14.

Types of grouping.

When the records were measured the position of each thrust point was taken, and also its angle with a horizontal line passing through the center. The subjects were rather inclined to group their records in a particular manner. Fig. 14 illustrates a type of grouping that was generally given by N. It is the same record as that shown in Fig. 13, and is what may be termed an *average* record. There are 5 minus and 5 plus, 5 right and 5 left thrusts, and the accuracy is fairly constant.

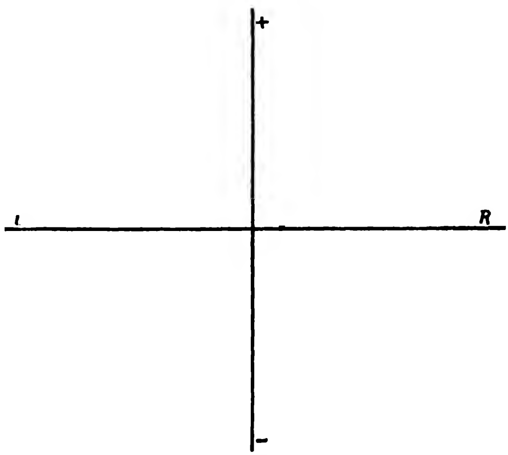


FIG. 15

Fig. 15 illustrates a different kind of grouping. With the exception

of the 5th, the thrust-points are in a line, extending from $R+$ to $L-$. Two of the subjects were quite apt to make such groups.

In Fig. 16 are represented both right and left records of W.A., who is equally proficient with both hands. The right hand thrust-points are indicated by crosses, the left by dots. Line AB is drawn from $R+$ to $L-$ a little to the left of the center. All the right hand thrusts, with one exception, are above to the left of AB. All the left hand thrusts, with one exception, are below to the right of this line. A similar arrangement of thrusts was made by the other skilled fencers, though not in so marked a degree.

The following facts may be noted from a study of the groups: (1) Most of the groups are similar to Fig. 16,—right hand thrusts to the

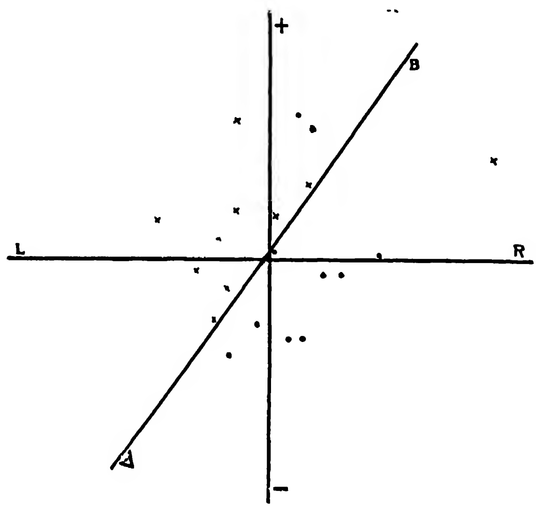


FIG. 16.

left of the horizontal line, left hand thrusts to the right. (2) The tendency to make such groups is more marked in skilled fencers. (3) The tendency increased in the six subjects during the practice. (4) The groups are most marked and distinct in the records of W.A., who has fenced with both right and left hands.

There is some evidence, then, to conclude that the influence causing the group records is unequal muscular development. The muscles chiefly used in the lunge are emphasized by practice and by their excessive contraction pull the arm to the other side of the body, thus producing a one-sided record. This unequal muscular development is transferred to the symmetrical muscles on the other side. The influence in

grouping is, therefore, peripheral and not central or mental. The chief influence at work in regulating the accuracy is central or mental.

V. Diversion of energy.

The following experiments were undertaken with the hope that they might help to furnish some explanation of cross-education.

Apparatus.

This was the same as that used in the tapping test first described, except that another key was clamped to the lap board, to allow one for each hand.

Method of experiment.

Records of five seconds of tapping were taken for the right hand, both tapping alone and also in connection with the other members. The records were taken in the following order: (*a*) direct, (1) right hand alone, (2) right and left hands together, (3) both hands and right foot, (4) both hands and both feet; (*b*) reversed, in the order (4), (3), (2), (1). In all cases only the right hand made a record. For each subject, two series of records were taken on each of four days. On the first and third days the records were taken in the direct order, and on the second and fourth days in the reverse order. When more than the right hand was tapping no attempt was made to have the different members tap with equal rapidity. The subject was instructed to devote as much attention to one member as to another and to tap as fast as possible with each. He did not know that the tapping of the right hand alone was being recorded. The seven subjects were Divinity and Graduate students of Yale University.

Results.

Table XVIII. gives the results obtained for the seven subjects. The figures here are made up from two averages. The average of two tests in the direct order was combined with that of two tests in the reversed order.

From the table it is evident: (1) that in general the right hand tapped more rapidly alone than in connection with the other members, (2) this difference in rapidity was not so marked on the third and fourth days as on the first and second. Even so short a practice made a striking change. In all cases, in the average of the first two days, the right hand was most rapid alone; except in the cases of H. and V., this difference was marked. In the average for the last two days, in all but two subjects, H. and V., the right alone was still more rapid though the difference in rapidity is small; on the contrary H. and V. were able to tap most rapidly

when all four members were tapping. The facts were generalized in the final averages. In the total average for the right hand alone the record was 36.9 for the first and second days, 36.3 for the third and fourth days, so that no gain in rapidity was made. The right hand's total average for tapping in connection with all the other members was for the first days, 29.7 and for the last, 34.7, a gain of 5 taps for 5 seconds, or 16%.

TABLE XVIII.

The influence of practice on automatic movements.

	1st and 2d days.				3d and 4th days.			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
H.	35	36	34	33	36	37	38	39
S.	27	28	21	18	35	32	32	33
T.	45	46	41	39	44	41	41	42
V.	38	35	33	36	33	34	35	36
X.	34	29	20	20	35	34	31	28
Y.	42	42	27	31	39	39	37	34
Z.	36	31	32	28	31	31	27	28
Total average	36 9	35.6	31 4	29.7	36.3	34.2	34.8	34.7
Relative average . . .	1.00	0.96	0.85	0 80	1 00	0.94	0 95	0 95

(1) Right hand alone (3) Right hand with left hand and right foot.
(2) Right hand with left hand (4) Right hand with left hand and both feet.

The last line of Table XVIII. shows the changes in rapidity that occurred with a practice of four days, giving the proportional rapidity of the right hand under the different conditions. In the tests of the first two days the right hand lost 20% of its speed while tapping with other members, in the last two days, only 5%. Fig. 17 represents the same facts in the form of a curve. *X* gives the number of members tapping, and *Y*, the number of taps in 5 seconds; while only the right hand is being recorded. The continuous line is the average of the first two days, the dotted line that of the last two days. There is a sharp descent in the continuous line, while the dotted line is more nearly straight. This indicates that practice has increased the ability of the right hand to tap in connection with the other members.

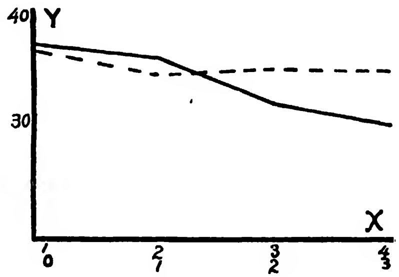


FIG. 17.

X, upper row, number of members used in tapping.

X, lower row, number of members in addition to right finger.

Y, number of taps in 5 seconds made by right index finger, the heavy line representing the average for the first two days of practice, the broken line that for the last two days.

With a longer practice the right hand, in multiple tapping, would undoubtedly excel in rapidity its record while tapping alone. This was true in the case of H. and V. who at first gave four records of nearly equal rapidity.

Regularity of tapping.

During the last tests of the experiment several of the subjects remarked that the multiple tapping had become easier, that they only needed to "set the machinery going and it went itself." The experimenter, listening to the tap counter, observed that in multiple tapping the right hand was more regular than when tapping alone. A specimen record is given to illustrate this fact.

	1st.	2d.	Average.	
(4)	39	36	37½	Tapping very regular.
(3)	36	35	35½	" " "
(2)	35	34	34½	Tapping irregular.
(1)	33	34	33½	" "

The complete record was taken in the reverse order, the observations appended being written down between the consecutive 5 second records. The irregularity is probably caused by too great concentration of energy or attention at one point—too much for the muscles to react to or properly dispose of. Hence the time was slower. It does not appear that the cultivation of the power to attend to several members at once is responsible for the increased rapidity in multiple tapping, since there was an actual decrease of attention in the later tests. For the same reason the increase of stored energy would not account for the fact. Very little energy is required during the short test, and one is able to tap continuously, without decrease in rapidity, for several minutes, while here there was a rest of 15 seconds between each 5 second record and the succeeding one.

The experiments of WELCH[1] give results similar to those described above. She concludes that rhythm of the left hand affects the right hand for the pull of the dynamometer, but that after practice this influence disappears. The explanation of such facts is probably found in the development of lower automatic centers by practice. After this development two acts, each of which at first requires conscious attention, become easy of simultaneous performance because the new automatic centers control the movements independently of the mind.

Fig. 17 exhibits the fact that in the first day's practice the right hand taps about as rapidly whether tapping alone or in connection with the

[1] WELCH, *On the measurement of mental activity through muscular activity and the determination of a constant of attention*, Amer. Jour. Physiol., 1898 I 283.

left hand, while much of its speed is lost while tapping with either one or both feet. This means simply that one arm is in closer nervous connection with the other arm than the upper limbs are with the lower.

VI. Theoretical.

An explanation of cross-education cannot as yet be completely and satisfactorily made. To aid in the explanation of the fact certain experiments of other observers are here brought together.

Experiments in diversion.

LOMBARD,[1] in his work with Mosso's ergograph, has shown that the strength of the right and left hands may vary either synchronously or independently. By means of two ergographs he secured records of both hands at the same time. He concludes that the variations are not due to any abatement of will power or attention, or to the fatigue of muscle; but rather to changes which affect the lower centers of the spinal cord. The variations in strength which occur synchronously are due to changes, probably circulatory, affecting large parts of the central system; those which occur independently are due to local changes.

BRYAN'S[2] experiments in tapping, carried on with children of different ages, prove that the effects of effort, through either extremity, are shared by both; and that the tapping ability of a joint is affected by the simultaneous exertion of the symmetrical joint on the other side.

PATRIZI[3] found that the strength of one hand varies whether it works alone or with the other hand. In his tests with the ergograph he found: (1) that in simultaneous action, the right hand could do 3.67 kgm. of work, and left, 2.94, total, 6.61; in alternative action, right, 3.72, left, 3.58, total, 7.30 (it should be noted that the gain in alternative action is made chiefly by the left hand, as if, in simultaneous action, more attention were paid to the right hand than to the left); (2) that with the left hand in isolated action, its work was 4.63 kgm.; and with the left working alternately with the right, its work was 5.64—a gain of 1.01 or 21.8%—showing that the action of the right hand reinforces that of the left.

---

[1] LOMBARD, *Some alterations in the strength which occur during fatiguing voluntary muscular work*, Jour. of Physiol., 1893 XIV 113.

[2] BRYAN, *On the development of voluntary motor ability*, Amer. Jour. Psych. 1892–93 V 201.

[3] PATRIZI, *La simultanéité et la succession des impulsions volontaires symétriques*, Archives Ital. de Biol., 1893 XIX 126, of which an abstract appears in the Année Psych., 1894 I 452.

The experiments of WELCH,[1] as well as my own already described in the section on *Diversion of energy*, reinforce these researches in establishing the very close nervous connection existing between the right and left limbs.

Experiments on fatigue.

In MOSSO'S[2] experiments with the ergograph, results appear to prove that muscular and central fatigue are quite distinct and independent. Fatigue, if considered as decrease in capacity for work,[3] is due almost wholly to influences that are central. After complete volitional fatigue, the muscle may be made to do much more work by electrical stimulation either of the muscle itself or of the nerve leading to it. LOMBARD,[4] and ROSSBACH and HARTENECK[5] support MOSSO in these conclusions.

Such considerations simplify the explanation of transference of practice; for if fatigue is chiefly central it is here that we must look for the most marked changes due to exercise. The effects of practice for the two hands, for example, are brought to an organ that has many things in common for both members.

Observations on attention and will power.

TOULOUSE[6] has proved the great importance of attention in acts of volition. The ability to pay attention decreases in all mental maladies. By means of the dynamometer he found that persons affected with diseases of the mind can exert only a feeble pressure, and he concludes that this inability is due to their lack of power to concentrate attention.

STUMPF,[7] in commenting on the investigations of FECHNER and VOLKMANN, described above on pages 6 to 7, makes the development of attention of great importance in transference. "The capability of concentrating attention on a certain point in question, in whatever field it is acquired, will show itself efficacious in all others." FECHNER, too, in this connection emphasizes attention.

---

[1] WELCH, *On the measurement of mental activity through muscular activity and the determination of a constant of attention*, Amer. Jour. Physiol., 1898 I 283.

[2] MOSSO, *Ueber die Gesetze der Ermüdung*, Arch. f. Anat. u. Physiol., Physiol. Abth., 1890 89.

[3] SCRIPTURE, The New Psychology, 228, London 1897.

[4] LOMBARD, *The effect of fatigue on voluntary muscular contractions*, Amer. Jour. Psych., 1890 III 24, also *Éffet de la fatigue sur la contraction musculaire voluntaire*, Arch. Ital. de Biol., 1890 II 380.

[5] ROSSBACH AND HARTENECK, *Muskelversuche von Warmblütern*, Arch. f. d. ges. Physiol. (Pflüger), 1881 XV 2.

[6] TOULOUSE, *Notes sur quelques expériences dynamométriques chez les aliénés*, Soc. de Biol , 1893 V 121.

[7] STUMPF, Tonpsychologie, 1883 I 81.

GILBERT and FRACKER's recent investigations[1] of reaction-time for sound prove that practice in reacting to one form of stimulus shortened the reaction-time for other forms. Like results were found for the ability of discrimination.

SCRIPTURE's[2] hypothesis for explaining cross education is "physiologically speaking, that the development of the center governing a particular member causes at the same time the development of higher centers connected with groups of members. Psychologically speaking, development of will power in connection with any activity is accompanied by a development of will power as a whole."

Observations on mirror-writing.

SALTMANN,[3] PIPER[4] and TREITEL[5] have investigated the subject of mirror-writing, both in normal and abnormal persons and in children and adults. Their results show a greater tendency to reversed writing in the young and in those suffering with nervous disorders or sensory deficiency, notably in the blind, deaf and idiotic.

GOLDSCHEIDER,[6] in his review of SALTMANN's article, explains mirror-writing as due to the greater influence in some persons of motor sensations over optical percepts. He considers that two directing factors must be distinguished in the production of writing; the optical percept of the written sign and the motor sensations involved in the movements. In mirror-writing the motor sensations correspond to those in direct writing, but the written signs do not correspond to the optical percepts. The sequence of innervation occurs in this case under the influence of the motor sensations which appear to be dissolved from the optical percepts. These latter form the principal factor in general and with normal persons; passing through the series of innervations according to motor sensations, represents a lower, partly mechanical type.

GOLDSCHEIDER's review apparently explains mirror-writing and also the transference of the effects of practice. If in certain persons the

[1] GILBERT AND FRACKER, *The effect of practice in reaction and discrimination for sound upon the time of reaction and discrimination for other forms of stimuli*, Iowa Stud. Psych., 1897 I 62.

[2] SCRIPTURE, *Recent investigations in the Yale Laboratory*, Psych. Rev., 1899 VI 165.

[3] SALTMANN, *Schrift und Spiegelschrift b. gesunden u kranken Kindern*, Festschr. z. Henoch's 70. Geburtstag, 432, Berlin 1890.

[4] PIPER, Schriftproben von schwachsinnigen Kindern, Berlin 1893, reviewed in Zt. f. Psych. u. Physiol. d. Sinn., 1893 VI 74.

[5] TREITEL, *Ueber das Schreiben mit der linken Hand und Schreibstörungen*, Deut. Zt. f. Nervenheilk., 1893 IV 277.

[6] GOLDSCHEIDER, Zt. f. Psych. u. Physiol. d. Sinn., 1891 II 414.

motor sensations have a greater influence than the optical percepts, then by the transferrence of the motor sensations a movement is made on the other side that is symmetrical to the movement practiced. This symmetrical movement produces the writing in the reverse direction.

Reflex action.

WALTON[1] has shown the intimate connection between the centers of motion and sensation and between the motor centers for different groups of muscles. The muscles of a frog under the influence of strychnine may all be put into a condition of tetanus by the stimulation of only one point of the skin. In ordinary reflex action the motor center for the muscle of the eyelid must be closely connected with the sensory center for the cornea, as the stimulation of one causes a contraction of the other.

HOFBAUER[2] has proved by means of the ergograph that a stimulation of the sense of hearing may excite a muscle to greater action. A pistol shot at just the right moment causes a higher contraction than ordinarily; but if it occurs a moment too soon or too late the contraction is hindered.

Observations on the overflow of energy.

The researches of EXNER[3] in reflex and cortical stimulation in the dog establish several important facts. (1) The two methods of stimulation may reinforce each other. (2) A cortical stimulation which concerns only the left foot reinforces the reflex act which, it might appear, concerns only the right foot and its central organ. That is, if the motor area for the left foot is stimulated electrically and at the same time the right foot is stimulated for reflex action, the movement produced in the right foot is greater than if only reflex stimulation is used. It was found, too, that cortical stimulation of the area governing one front foot reinforces reflex action in the hind feet, and likewise the reverse.

URBANTSCHITSCH,[4] in his experience with pathological subjects with diseases of the eye and ear, has found true for the sensory nerves what EXNER found for the motor nerves. Sensations, as well as motor impulses to action, may affect parts seemingly not immediately concerned.

---

[1] WALTON, *Ueber Reflexbewegung des Strychninfrosches*, Arch. f. Anat. u. Physiol., Physiol. Abth., 1882 46.

[2] HOFBAUER, *Interferenz zwischen verschiedenen Impulsen im Centralnervensystem*, Archiv f. d. ges. Physiol. (Pflüger), 1898 LXVIII 546.

[3] EXNER, *Zur Kenntniss von der Wechselwirkung der Erregungen im Centralnervensystem*, Archiv f. d. ges. Physiol. (Pflüger), 1882 XXVIII 487.

[4] URBANTSCHITSCH, *Ueber den Einfluss von Trigeminus-Reizen auf die Sinnesempfindungen, insbesondere auf den Gesichtssinn*, Archiv. f. d. ges. Physiol. (Pflüger), 1883 XXX 129.

An hour's exclusive operation on the right eye showed on the left a relative enhancement of the ability to see. In many patients with chronic catarrh of the middle ear the observer was surprised to find that an important pathological influence was transferred from the ear to the sense of sight.

DAMSCH[1] explains the spreading of nervous impulses to action as due to the close connection of all motor centers. Impulses from the motor centers tend to spread themselves by their entrance into the great central brain ganglion—where fibers from all motor centers come together and are intimately connected. This spreading of impulses is hindered by a checking, or inhibiting, apparatus which keeps the impulse from going the wrong way. This apparatus is much improved by practice. In the young and in certain nervously disordered persons it is deficient.

It was noticed in the tapping experiment that there was a tendency at times for the subject's left foot to make movements to accompany those made by the right. DAMSCH'S explanation for such movements is that the impulse sent to the right foot has in part escaped through the checking apparatus and gone the wrong way. In learning an act that involves fine coördination it is very obvious that the pupil executes many movements that are entirely unnecessary. The nervous impulse has flowed out into wrong channels. These observations show how closely related are the motor centers governing symmetrical or associated parts and how an influence sent out from the central nervous system to a peripheral organ may be felt in other peripheral organs also.

Conclusions.

The following conclusions may be drawn from my own experiments and those of other observers.

a. The effects of exercise may be transferred to a greater or less degree from the parts practiced to other parts of the body. This transference is greatest to symmetrical and closely related parts.

b. There is a close connection between different parts of the muscular system through nervous means. This connection is closer between parts related in function or in position.

c. Will power and attention are educated by physical training. When developed by any special act they are developed for all other acts.

Explanation of cross-education.

With conclusions *b* and *c* established the explanation of transference is

[1] DAMSCH, *Ueber Mitbewegungen in symmetrischen Muskeln an nicht-gelähmten Gliedern*, Zt. f. klin. Med., 1891 XIX 170.

probably reached. There is no doubt that the most important effects of muscular practice are central rather than peripheral. The central effects may be distinguished as: (1) those dependent on the development of motor centers, that is, their improvement through exercise; (2) those dependent on the development of psychical factors, notably attention and will power. Of these two effects we would emphasize the first as the more important. In fact, in the tapping tests close attention and a strong will power were hindrances. In tests requiring strong effort these factors are useful.

With the improvement by exercise of the motor centers governing the right arm, there is through the close nervous connection an improvement also of the center governing the left arm. Besides this in tests where will power and attention are necessary, these elements are developed by the exercise of the right arm and are efficacious also for the left.

The peripheral effects of exercise cannot be ignored altogether. It has been noted before that in the dumbbell test, the left arm did no improve, relatively to the right, to so great a degree as it did in the other tests. It did not gain endurance to an extent comparable to the gain in the right. In purely muscular tests it is necessary not only to develop the center of motor control, but also to develop muscular tissue. The muscles must be put into better condition to gain endurance. It has been seen that the left arm gained in girth and to a varying extent in the power of endurance. This can be explained only by increased muscular nutrition.

The measurements to determine if the circulation varied alike in both arms when only one was exercised, gave negative results. That the centers governing the nutrition of the right and left arms are affected alike by the exercise of either arm, suggests itself as a probable reason for the increase in girth in the arm not used.

RESEARCHES IN PRACTICE AND HABIT,

BY

W. SMYTHE JOHNSON.

I. TRIANGULAR MOVEMENT.

The subject was required to tap continuously at the corners of an equilateral triangle whose sides measured 20^{cm}. This triangle was formed by a special triangular contact key, Fig. 1, with knobs 20^{cm} from each other. This key was originally constructed at Dr. SCRIPTURE'S suggestion for use in testing school-children by GILBERT, by whom, however, it was used merely for tapping and not as a habit-key.[1]

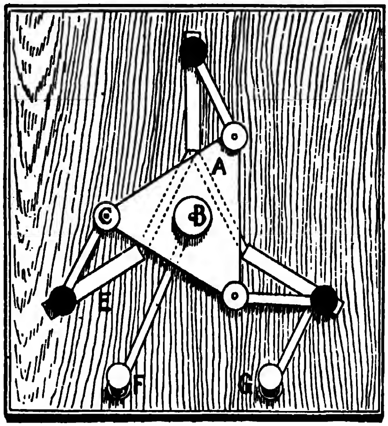


FIG. 1.

The key was placed in circuit with a 4 ampère battery and the primary coil of a spark coil, the condenser being connected around the break. From the poles of the secondary coil, one wire led to the base of the recording drum, the other to the base of a 100 v.d. electric fork bearing a flexible point on one of its prongs. Pressure on one of the key-knobs closed the primary circuit for an instant. When the circuit was broken a spark passed through the smoked paper on the surface of the drum making a dot on the time line drawn by the fork. Each spark thus indicated a tap on one of the three key-knobs. The time between the sparks could be read to the thousandth of a second.[2]

The subjects included: K. (Kochi) and M. (Matsumoto), who were Japanese students of psychology; P. (Powell), a student of English;

---

[1] GILBERT, *Researches on the mental and physical development of school-children*, Stud. Yale Psych. Lab., 1894 II 40 (especially p. 40 and Fig. 5)

[2] The arrangement of the recording apparatus was identical with that of Exercise IX in SCRIPTURE, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 113; it is shown in SCRIPTURE, New Psychology, frontispiece, London 1897.

L. (Lloyd), a student of philosophy; H. (Hawkins), a student of divinity; S. (Dr. Scripture); and J. (Johnson).

Before beginning the experiments the subjects were requested to devote their attention and energy to the exercise throughout each experiment and always to make as fast a record as possible. They were also instructed to practice no similar movements at any time outside that of experimentation. They were not allowed to inspect their records nor were they given the least intimation whether they were gaining or losing; exceptions to this rule occurred in the cases of J. and S.

The person experimented upon was placed in a quiet room.[1] The triangular key lay on a table. The subject stood while performing the experiment, as it was found that this position allowed the freest movement of the forearm. He fixed his gaze on the key. Before beginning an experiment a preliminary trial was allowed in order to acquaint the fingers with the relative positions of the keys. The subject was also asked to endeavor to regulate the tension of his muscles commensurate to the strain of tapping fifty successive times. Unless thus cautioned, he was almost certain to break down before the end. If he did not break down completely, he would at least miss some of the knobs in making the succession of movements. The caution was given only at the beginning of the whole set of experiments as it was merely intended to prevent the adoption of too high a standard in the beginning. It was not again mentioned lest the mental standard chosen in the first experiment should be changed. It was found that the subjects followed this request with respect to the left hand more closely than with respect to the right hand.

With all the subjects except S. and J. the experiment was first performed with the right hand, then with the left, and again with the right. In the case of J. only the right hand was used and in that of S. only the left hand.

Fatigue showed itself (1) when the subject completely broke down, (2) when he struck the knob of the key so inaccurately that he knocked it out of place, (3) when he missed one of the knobs of the key. The last case was the usual one and when it occurred the experiment was considered to have ended. By counting the records to this point we have a result practically free of fatigue.

Daily average.

The average tapping time for each day is shown in Table I. Curves

[1] Described by SCRIPTURE, New Psychology, 136, London 1897.

corresponding to the table are presented in Figs. 2, 3, 4, 5, 6. A comparison of these curves shows that, while each takes a direction determined by individual characteristics, yet they all follow closely the same law of gain.

TABLE I.

Average interval between taps on successive days.

Subject.	Hand used.	1	2	3	4	5	6	7	8	9	10	11
K.	*R*	308	283	280	277	268	242	241	213	212	209	203
	L	300	292	316	278	276	258	243	239	234	230	226
M.	*R*	294	314	302	287	271	281	242	239	235	229	206
	L	314	321	302	288	262	281	274	273	253	250	187
S.	*L*	219	235	217	201	197	188	185	182	172	162	
J.	*R*	224	219	210	206	230	203	195	191	171		
P.	*R*	244	196	175	157	152	143					
	L	226	203	173	198	186	174					
L.	*R*	245	210	194	174	169	157					
	L	261	244	223	199	192	186					
H.	*R*	212	169	159								
	L	240	174	172								

The unit of measurement is $1\sigma = 0.001^s$.
The number of measurements in each case was from 40 to 50.
The probable errors of the determinations vary from ± 5 to $\pm 1\sigma$.

An omission of one or more days did not in every instance materially effect the amount of gain resulting from practice. An interval of two days between the first and second experiments of P. and L. did not seem to effect their records; an interruption occurring between the fifth and sixth days with the subjects K. and M., and between the fourth and fifth days with J. and S., showed its effect on J. and M. very clearly, but in only a small degree on K., and seemingly not at all on S.

The losses on the fourth, fifth and sixth days with P. (left hand) may be attributed to the fact that he unthinkingly reversed the original direction of the movement in using the left hand. In the three previous exercises he had made his hand move clockwise. All the subjects except J. were right-handed and the right hand was always moved counter-clockwise, the left moving clockwise. The hands were thus moved symmetrically but in opposite directions. The subject J., however, moved his left hand counter-clockwise and his right hand clockwise. This would seem to indicate that the centers governing the rotary movements of the muscles in right and left handed persons are diametrically opposed

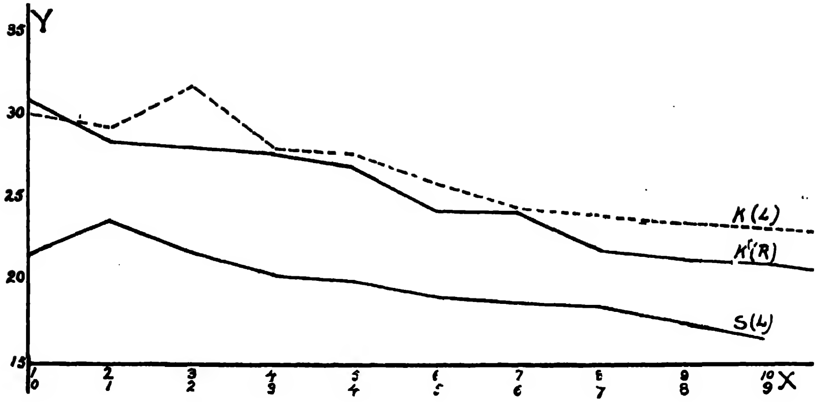


FIG. 2.

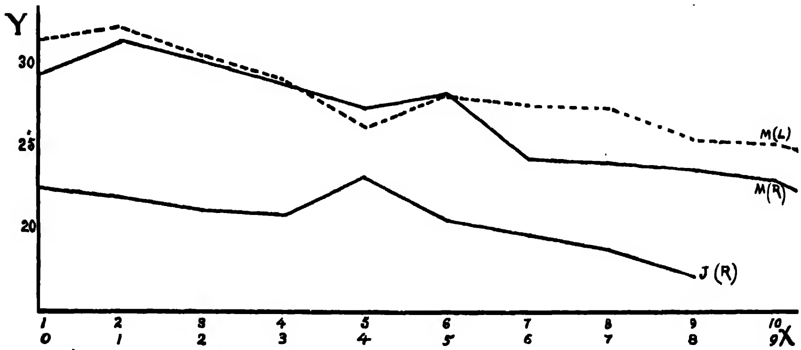


FIG. 3.

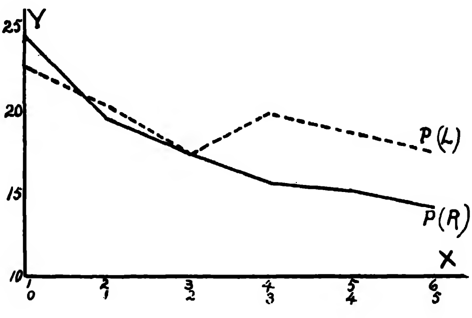


FIG. 4.

X, upper line, serial number of experiment.
X, lower line, days of previous practice.
Y, tap time in thousandths of a second.

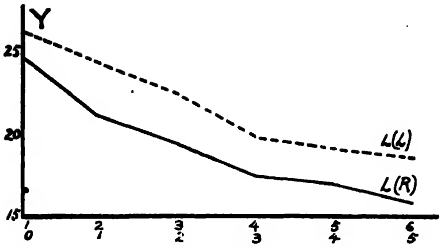


FIG. 5.

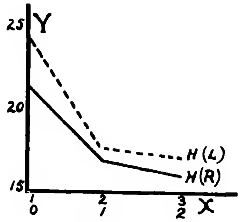


FIG. 6.

X, upper line, serial number of experiment.
X, lower line, days of previous practice.
Y, tap time in thousandths of a second.

to each other. A few experiments were made on another left handed person; the same direction of the movement of either hand was chosen as with J. However, I caused him to reverse the direction of the movement of the right hand so as to move counter-clockwise. As a result the movements of the right hand were much slower and fatigue set in earlier. Moreover, there was less regularity in the movements of the right hand.

Daily probable error.

The change in the probable error for successive days, given in Table II., may be considered as an expression of the development of automatic control over the movements of the hand.

TABLE II.

Probable error on successive days.

Subject.	Hand used.	Serial number of experiment.										
		1	2	3	4	5	6	7	8	9	10	11
K.	R	50	38	23	36	32	43	37	15	15	17	20
	L	80	51	18	38	30	29	20	16	27	40	18
M.	R	30	37	26	47	29	36	20	20	20	14	13
	L	35	38	19	20	24	30	22	26	27	17	42
S.	L	39	31	37	27	31	28	37	39	27	27	
J.	R	39	36	48	26	35	42	29	33	30		
P.	R	30	18	18	20	15	14					
	L	26	23	19	18	15	15					
L.	R	35	30	29	21	17	20					
	L	31	28	24	23	19	13					
H.	R	36	21	27								
	L	27	27	32								

The unit of measurement is $1\sigma = 0.001^s$.

This table gives the probable error of the tapping times from which Table I. was computed.

The probable error P was calculated according to the formula

$$P = \tfrac{2}{3}\sqrt{\frac{v_1^2 + v_2^2 + \ . \ . \ . \ + v_n^2}{n-1}}$$

where $v_1, v_2, \ldots, v_n$ are the errors for the n measurements. Table II. shows a decrease from day to day which closely corresponds to the average daily decrease in the intervals between taps.

An average of the decrease in the probable error of all the subjects for

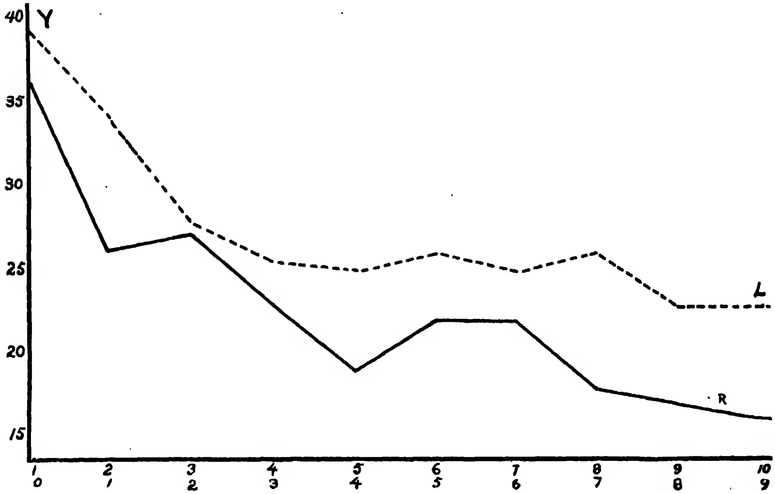


FIG. 7.

X, upper line, serial number of experiment.
X, lower line, days of previous practice.
Y, tap time in thousandths of a second.

each hand was made; a graphical representation of it is given in Fig. 7. Though the error for the left hand was larger than for the right, the curve for either hand takes about the same direction. Hence the centers governing the left-hand movements, though less developed, are susceptible to the same law of gain in automatic control.

The irregularities in the daily decrease of the error may be accounted for in part by the variations in the nervous condition of the subject from day to day. Moreover, the preliminary trials given just before beginning each experiment, which were always the same in number, were not sufficient in every case to arouse the nervous centers so as to get the best results from practice, for I observed that in a few instances the tapping time was very slow at first, but rapidly increased in speed during the first part of the experiment, which caused a larger probable error. Thus

the 43^{σ} probable error of K. on the sixth day was principally due to the long intervals between the first four taps.

That the preliminary trial was not sufficient in every instance for him to acquire momentum, so to speak, is seen in the record for the sixth day, which ran as follows: 315, 289, 291, 295, 200, 245, 244, 240, 269, 242, 248, 251, 243, 237, 246, 238, 254, 240, 229, 254, 218, 219, 260, 222, 231, 239, 235, 216, 223, 242, 229, 217, 212^{σ}. This shows a difference of 103^{σ} between the first and last taps, though the largest decrease occurred in the first four taps. As contrasted with this record, we give his record for the eighth day, which shows considerable gain in rapidity and regularity in two days' practice: 231, 229, 225, 220, 219, 223, 229, 226, 223, 221, 218, 222, 208, 210, 221, 226, 209, 204, 229, 221, 203, 207, 203, 200, 198, 217, 213, 209, 205, 202, 200, 195, 208, 203^{σ}. This record shows a difference of only 29^{σ} between the first and last taps. A graphical representation of the records for these two days may be seen in Fig. 8. According to the statement of the subject on the

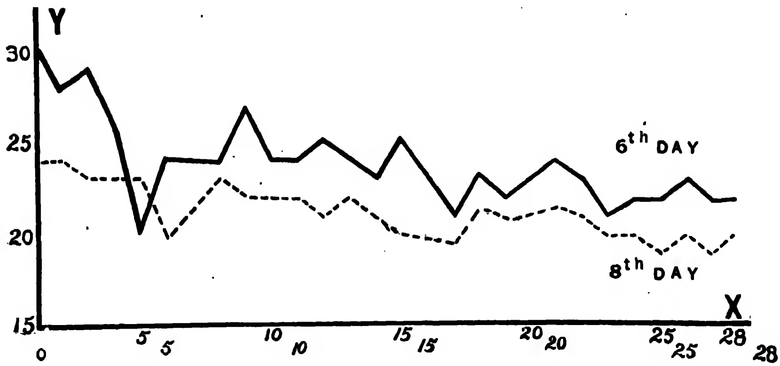


FIG. 8.

X, upper line, serial number of experiments.
X, lower line, days of previous practice.
Y, tap time in thousandths of a second.

sixth day, he was conscious at the start that he was not doing his best, but somehow he could not get control of the muscles within the time allowed him in the preliminary trial. Hence the temporary effect on the first part of the record.

In those records which show a small variability the subjects invariably reported that they were feeling quite well and brisk. Moreover, I observed that as the movements came to be controlled by the automatic centers and the movements became more rapid, the greater was the influence of any change in the nervous condition of the subject. An illus-

tration of this may be seen in the results for J., sixth day, where he reached the highest probable error of his whole set of experiments, having inadvertently received an electric shock just before beginning the experiment.

The rapid decrease in the intervals between taps with P. (right hand, Fig. 4) emphasizes the influence which previous training of any one set of muscles has upon the formation of automatic control where the form of movement is changed only as to direction. It is even more manifest in the decrease of the probable error. Although the movements involved in this exercise were different from those required in piano playing, yet the years of previous training had developed the centers controlling the movements of the hand to respond quickly and with considerable regularity ; so when the movements became circular (as was necessary in tapping the three keys) it was only necessary to establish the one additional element in the automatic centers, the direction of the movement of the hand.

Averages of successive taps.

My observations during different experiments each day convinced me that the subject gained in rapidity of tapping constantly from the beginning of the experiment until the close or until the setting in of fatigue. I tested and corroborated my conviction in the following way: Reviewing the protocols, I derived an average from all the first taps, then from all the second taps, and so throughout the whole series. That my conviction was correct is clearly shown by the fact that in every case the tapping time was decreased, ranging from 30^σ to 100^σ.

Considering the large number of experiments made and the number of subjects included in these tests, the constant increase in rapidity of muscular action during each experiment exceeded our expectations. Indeed, it may be stated as a law of practice wherein rapidity is the objective point, the movement does not follow any rhythmical law of increase and decrease, but constantly increases in speed until the setting in of fatigue.

A few experiments were made wherein the tapping time was long continued, which showed that when fatigue first sets in, the subject loses for a short time, then there is a renewal of effort and the speed is constantly accelerated for a certain period, but not so long as in the first. As each successive period of fatigue came on, the successive period of increase in speed was shorter; this was continued until that state was reached where the alternations were so rapid as to effect almost every other tap. This accords with the assertions of other investigators who have directed their investigations especially to the study of fatigue.[1]

[1] MOORE, *Studies of fatigue*, Stud. Yale Psych. Lab., 1895 III 68; BINET ET VASCHIDE, *Expériences de vitesse*, Année psychol., 1897 IV 267.

The lack of the development of the centers governing the movements of the left hand not only caused the tapping time to be slower than for the right hand, but the amount of gain during each experiment was also less. For instance, the average amount of gain for the right hand of K. was 90^σ in 20 taps, while with the left he gained only 40^σ. Stated in words, the gain with the left hand of all of the subjects was much less in proportion to the rapidity of its movement.

Although P. was the most rapid, yet he gained less in amount than the other subjects during each experiment, for the right hand gained only 50^σ and the left possibly 10^σ. This demonstrates the fa t that, as the muscles come more completely under control, the influence of practice becomes less during each experiment. With L., however, the results for the left hand are more favorable. namely, a gain of 40^σ with the left hand, compared to a gain of 50^σ with the right. The results of H. are exceptional in that the gain was equal for both hands, namely, only 30^σ. With the subjects K., M. and P. the centers governing the left hand were slower in their activity and less influenced by training than with L. and H.

In like manner, the probable error for successive taps was determined. It showed a tendency to decrease as the experiment progressed. If the subject put forth the greatest effort in the beginning of the experiment, the error was correspondingly greater in the first part. But when the special effort was relaxed, the muscles reverted to their more accustomed speed of adjustment, and at the same time became more regular in their functioning. Therefore, a minimum gain in rapidity during any practice period is the best condition for impressing that standard upon the nervous centers; as a result the decrease of the probable error is correspondingly accelerated. This principle is well illustrated in the results obtained for the subjects J. and P. both of whom gained very little in rapidity of tapping during the progress of an experiment, but who, however, made considerable decreases in the probable error. As long as spasmodic accelerations are observable throughout a short series of practice, the automatic control over the muscles may be considered imperfect, the degree of imperfection being indicated by the amount of the probable error.

Relative gain by practice.

Although the gain from day to day varies in response to the mental and physical condition of the subject, yet we may suppose from the conformity of results of all the subjects that the acquirement of muscular facility, or, physiologically, the transition from that state which demands

constant fixation of the attention to the state of automatic control follows as closely a mathematical law as do falling bodies. The difficulty in finding such an expression is partly due to the difficulty in getting the subject under the same physical and mental conditions at each experiment and partly to the lack of scientific results on allied subjects upon which the law of habit must also depend. For instance, some of the personal factors of our problem causing variations in the results obtained from different individuals are: differences in muscular memory for different individuals; different physical conditions due to differences in the constituents of the blood, etc.; the rapidity of the heart-beat; the temperature of the body; the power of the fixation of the attention; the interest in the experiment; and the influence of emulation. The more nearly these various conditions approach the normal and the more accurate the measurements are, the nearer will the results for different individuals conform to the same law of gain in rapidity and regularity of muscular adjustments. This law of development may receive mathematical expression either by percentages or by algebraical formulæ. Of these two methods, I have adopted the former. I have adopted a method somewhat similar to that used by AMBERG[1] for determining the percentage of gain by practice. It will be seen, however, that I do not adopt his method without modification.

We took the average tapping time at the first experiment as a measure of the subject's ability without any previous training. The difference between the averages of the first and second experiments was taken as representing the influence of practice during the first experiment. Likewise, the average tapping time of the third experiment was subtracted from the average of the first. This process was repeated throughout the series. Then taking the sum of the whole number of gains over the average of the first experiment and dividing by the number of experiments, we obtained the average gain for the whole series over the results of the first experiment, which was expressed as a percentage of the average tapping time at the first experiment. But this did not complete the series, for at the succeeding experiment he began the practice anew just as at the first experiment. It was, therefore, necessary to repeat the same calculation, taking the average tapping time at the second experiment as the basis. In like manner, we continued the process for the whole number of experiments, thus obtaining the average percentage of gain, taking each succeeding experiment as the beginning of a series. Then dividing the sum of these percentages by the number of experiments

[1] AMBERG, *Ueber den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit*, Psychologische Arbeiten (Kraepelin), 1896 I 30.

we obtained the average percentage of gain for the whole series of experiments.

The formula for the computation of these percentages may be stated in the following way.

Let the averages for the 1st, 2d and 3d, ⋯, nth days be a, b, c, ⋯, l. Then take

$$(1)\quad \begin{aligned} (a-b)+(a-c)+(a-d)+\cdots+(a-l) &= A,\\ (b-c)+(b-d)+\cdots+(b-l) &= B,\\ (c-d)+\cdots+(c-l) &= C,\\ \vdots \qquad &\ \ \vdots\\ (k-l) &= K, \end{aligned}$$

$$(2)\quad \begin{aligned} A \div (n-1)\,a &= M\%,\\ B \div (n-2)\,b &= N\%,\\ C \div (n-3)\,c &= O\%,\\ \vdots \qquad \vdots \quad &\quad \vdots\\ K \div k \quad &= Z\%\,; \end{aligned}$$

$$(3)\quad \frac{A+B+C+\cdots+K}{(n-1)a+(n-2)b+(n-3)c+\cdots+k} = \text{Ave. }\%.$$

The signification of such percentages is that they give us a true standard for the comparative influence of practice on different individuals. Although all practiced the same amount each day under similar conditions, yet we shall now see how differently the percentage of gain in speed of voluntary movements differed with each subject from day to day and how similar were the final results after the completion of the entire series.

TABLE III.

Relative gain in speed from day to day.

Subject.	Hand used.	1	2	3	4	5	6	7	8	9	10	Ave.
K.	*R*	0.21	0.16	0.17	0.18	0.18	0.11	0.13	0.02	0.03	0.03	0.12
	L	.14	.12	.21	.12	.13	.09	.04	.04	.03	.02	.09
M.	*R*	.12	.19	.17	.16	.12	.18	.06	.07	.07	.10	.12
	L	.16	.18	.14	.12	.05	.12	.12	.16	.14	.25	.14
S.	*L*	.11	.20	.15	.10	.10	.06	.07	.08	.06		.10
J.	*R*	.09	.08	.05	.04	.17	.08	.07	.10			.08
P.	*R*	.32	.20	.14	.06	.06						.16
	L	.17	.10	—.07	.09	.07						.07
L.	*R*	.26	.17	.13	.07	.07						.14
	L	.20	.18	.14	.05	.03						.12
H.	*R*	.22	.06									.13
	L	.28	.01									.14

The values in columns 1, 2, ..., 10 were calculated by formula (2), those in the column Ave. by (3). The figure for any particular day indicates the combined relative gain for all succeeding days over the record for that day.

The relative average daily increase in speed is given in Table III. For the right hand of K. the average percentage of gain on the second day was 21% over the speed made at the first experiment. But, as is shown in the table, the percentages decreased perceptibly until the close of the series of experiments, ending on the eleventh day with a gain of only 3% over the speed of the tenth day. The small percentage in the latter part of the series would seem to indicate that he had approximately reached his limit in rate of movement. So with all the other subjects, the percentage of gain in increase of speed constantly declined as the practice was continued from day to day. The large percentages in the first part of the series show that the greatest gains are to be made in the early part of practice.

The average percentage of gain given in the column of averages shows the comparative value of practice for each individual. Comparing K. and M., whose experiments extended over the same number of days, we see that practice had the same effect on the right hand of each of them, namely, 12%; but for the left, K.'s average percentage of gain was less than for the right hand, while with M. it was even larger than for the right. So with S. and J., we see that practice was of more value for S. than J. by 2%. Likewise P. gained 2% more with his right hand than did L.; but with the left hand L. gained 5% more than did P.

The average daily decrease of error was also derived according to the formula given on page 61; the results are given in Table IV.

TABLE IV.

Relative average daily decrease of error.

Subj.	Hand used.	Serial number of day. 1	2	3	4	5	6	7	8	9	10	Ave.
K.	*R*	0.45	—0.22	0.28	0.27	0.23	0.51	0.55	—0.16	—0.23	—0.18	0.15
	L	.64	.42	—.68	.29	.11	.10	—.27	—.9	—.7	.55	.10
M.	*L*	.13	.33	.05	.54	.30	.52	.16	.21	.32	.07	.26
		.25	.33	—.37	—.33	—.14	.11	—.27	—.11	—.9	—.59	—.11
S.	*L*	.19	—.02	.17	—.17	—.02	—.13	.16	.31			.05
J.	*R*	.11	.04	.32	—.30	.04	.27	—.09	.09			.07
P.	*R*	.43	.07	.09	.27	.07						.19
	L	.32	.26	.23	.17	.00						.19
L.	*R*	.38	.27	.33	.12	—.18						.18
	L	.31	.29	.23	.30	.32						.29
H.	*R*	.33	—.29									.02
	L	—.09	—.16									.11

The explanation is the same as for Table III.

Comparing the decreases of the probable error for K., 15%, and M., 26%, we see that practice was more beneficial for the right hand of M. than

for the right hand of K. But for the left hand the reverse was true. Even if we cast out the last practice of M., on account of its poor effect, the — 11% changes to + 6%. It should be noted that the percentages for the left hand of K. and M. are the reverse of those given in Table III. Likewise the same is true for S. and J., for while S. gained more in speed, J. gained more in regularity of movement. The small percentage of decrease of the probable error would seem to indicate that S. and J. kept the order to tap as quickly as possible in the foreground of the attention, for the percentage of gain in rapidity was larger for the whole series with each of them than was the reduction of the probable error. On the contrary with the subjects K. and M. who had approximately reached their utmost speed of voluntary movement, the percentage of the decrease of the probable error was larger than the percentage of gain in speed.

We may summarize the results given in Tables III. and IV. in the following way: P. made the greatest percentage of increase in speed with the right hand, and S. and L. the greatest with the left; that of all the subjects J. possessed least ability for development of rapid movements with the right hand. When regularity, not rapidity is considered, the right hands of K. and M. and the left hands of K. and L. made the greatest gain in regularity of movement while S. made the least of all.

Relative average daily gain in speed and decrease of error compared.

The average percentage of increase in speed and of decrease in the probable error from day to day for all the subjects were compared; the results are expressed in the A and B curves of Fig. 9. These curves

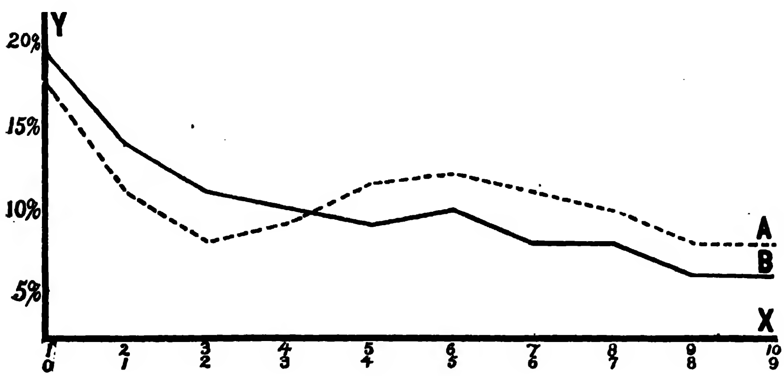


Fig. 9.

X, upper line, serial number of day.
X, lower line, days of previous practice.
Y, relative increase in speed (B) and decrease in probable error (A).

show very clearly that whereas the percentage of gain in rapidity of movement is larger in the first part of the series of the experiments, in the latter part the greatest influence is directed toward the reduction of the irregularity of the movements.

Considering the curves A and B as representatives of the influence of practice on seven subjects, indicating the gain made in six to eleven days, we are justified in making the following general statements concerning muscular action.

First, the gain in rapidity and the gain in regularity of muscular action are greatest during the first periods of exercise; one or both of these continues to diminish as practice continues at each experiment as well as at successive experiments.

Second, during the earlier periods of exercise practice has greater effect upon the rapidity of muscular action, but later its greater effect is in the reduction of the irregularity of voluntary movement.

Third, the relations of rapidity and irregularity are largely affected by the relative complexity of the muscular movements, the number of muscles undergoing training, and the subject's power of concentration of attention.

II. Drawing circles.

The object of this set of experiments in drawing circles was to show: (1) the gain in the reduction of imperfections in the drawings during each experiment and from day to day; (2) the influence that a copy placed before the subject had upon the size of his drawings.

A package of ten sheets of paper, 125^{mm} by 100^{mm}, was placed before the subject, sitting at a table. A sheet of paper with a circle of 60^{mm} in diameter, described with a compass, was placed before him as a copy; he was asked to look carefully at it each time before commencing to draw his circle. The copysheet was the same size as those used by the subject. The ten slips of paper lay one upon the other at the start; the experimenter stood by and removed them as the circles were drawn. Thus all visual comparison with previously drawn circles was prevented. It was impressed upon the subject that he should keep the true circle constantly in memory after he took his eyes off the copy to direct his hand. The circles were drawn with the free hand, no portion of the hand or arm being allowed to rest on the table during the process. No restrictions were imposed on the time consumed in the drawing of the circles. All distracting influences were removed. Before beginning, the subject was enjoined to do his best at each trial.

The subjects were J. F. and S. F., teachers in the primary grades of

the New Haven city schools, who had previously had considerable practice in drawing circles in the school-room ; M. C., a girl of twelve ; B., C., H. and J., students in the university.

A characteristic expression for the deviation of the drawn curve from the true circle may be found by comparing the longest diameter with the shortest one. The two diameters were measured in millimeters and their difference was considered as the amount of error.

It was deemed advisable to limit the number of circles drawn at one sitting to ten, so that the element of fatigue might be almost if not completely eliminated. Preliminary experimentation showed that the influence of fatigue was not perceptibly present with the right hand until the ninth or tenth repetition, and with the left hand until the fifth to seventh repetitions. However, those curves in the series which most nearly approximated true circles resulted in attempts ranging from the fourth to the seventh. Consequently, under ordinary circumstances, the drawing of five circles would give sufficient practice for the subject to reach an approximate maximum of accuracy of adjustment. Hence it was deemed best to stop the experiment at a point where the subject was gaining in accuracy rather than to continue it until the errror began to increase.

Gain on successive days.

The average errors for each day are shown in Table V.

TABLE V.

Average error on successive days.

Subject.	Hand used.	Serial number of experiment. 1	2	3	4	5	6
B.	*R*	12	9	12	11	8	9
	L	10	9	7	5	5	8
M.C.	*R*	10	9	7	7	6	5
	L	13	10	7	6	5	4
C.	*R*	6	6	5	4	4	3
	L	10	9	9	9	8	7
J.F.	*R*	6	6	5	5	3	2
	L	11	10	9	8	8	4
S.F.	*R*	6	6	6	5	4	4
	L	7	6	5	5	4	4
H.	*R*	9	9	8	7	7	6
	L	7	8	10	8	9	10
J.	*R*	11	6	5	5	4	3
	L	9	13	8	12	7	6

As is shown in these figures, practice with the left hand gave more irregular results than that with the right. However, B.'s record was an exception in that the error was not so large as that for the right hand. Moreover, the error decreased more rapidly in the practice with his left hand. The small and irregular gain with his right hand may have been due to a tendency to accomplish the task with great rapidity and to previous practice in writing, for his penmanship was sharp and pointed. Hence the left hand had the advantage over the right since the right hand not only had to form a habit for a certain movement but to reform one.

With H. and J. the lack of improvement with the left hand was due to a decrease of effort. I observed that they made great effort on the first day; but after that they declared it useless for them to practice with the left hand because improvement was hopeless. These two records plainly demonstrate the influence of confidence in one's ability upon one's development. Purposeful attention and persistent effort on the part of the subject are the two most essential elements in practice for the establishment of any definite mode of muscular action.

Though the amount of gain with C. was the same for each hand, yet the magnitude of the error at the beginning of the series with the left hand gave more scope for improvement, and had the subject been equally skillful with his left hand the curve would have descended much more abruptly. On the contrary, M.C. gained more with the left hand than she did with the right. The very regular decrease of the error with this subject from day to day shows the influence of persistent effort; I observed that she was very careful throughout the whole set of experiments.

Though there was a gain of 7^{mm} for the left hand of J.F. as contrasted with a gain of 4^{mm} for the right, yet most of the decrease in the error for the left hand occurred at the last experiment, which was undoubtedly due to the extra effort put forth. Though the subject thought all along that she was doing her best while practicing with the left hand, yet after the close of the series she said that she was conscious of having made considerably greater effort at the last experiment. None of the subjects save J.F. knew that the sixth day's trial would close the set of experiments.

Gain in successive circles.

The average of the errors for all first circles, that for all second circles, etc., were taken. The results, Table VI., show that with the most of the subjects the error became less as the practice was continued.

TABLE VI.

Average error for successive circles.

Subject.	Hand used.	Serial number of circle. 1	2	3	4	5
B.	R	11	10	9	9	10
	L	8	5	9	7	9
M.C.	R	10	9	8	7	7
	L	7	6	6	7	8
C.	R	6	5	5	5	4
	L	10	9	9	8	7
J.F.	R	6	5	5	4	3
	L	9	9	8	7	7
S.F.	R	6	6	5	5	3
	L	7	6	6	4	4
H.	R	8	7	7	6	6
	L	9	8	7	9	10
J.	R	7	6	6	5	4
	L	6	9	10	8	10

The unit of measurement is 1^{mm}.
Each figure is the average of 6 experiments.
The probable error of a determination varies from $\pm 0.9^{mm}$ to $\pm 0.1^{mm}$.

It has been proposed by Dr. Scripture to call the curve of change for a single continuous experimental session the "curve of practice" and the curve of change for successive sessions the "curve of habit." In the present case the gain on successive days would be represented by a curve of habit and the gain in successive circles by a curve of practice.

In the case of M. C. the practice and habit curves are alike in that they each begin with an error of 10^{mm} and end with 7^{mm}. The maximum decrease of error was not reached until the fourth circle was drawn with the right hand, while it was reached in the second with the left. The control of the movements of the left arm was evidently not sufficiently developed to prevent fatigue during the time required to make five circles.

The similarity between the practice and habit curves seems to indicate that the development during each practice period follows closely the same law as does the daily progress, for in both alike the gain is most rapid in the first part of the exercise. However, the amount of gain for the different subjects varies considerably; this I attribute to the differences in the muscular memories of different individuals. With some, the error of the first circle on successive days was only slightly larger than the average for all the circles made on the preceding day, while with others it was in some instances equal to that at the beginning of the preceding experiment but rapidly decreased. The law of practice and the law of

habit are presumably of the same general form with different constants for different circumstances.

Comparison of the drawn curve with the original circle.

In order to indicate the size of the curve drawn as a circle, four diameters were measured and their average taken. The diameters measured were the shortest, the longest and the two 45° from these. These measurements furnished an approximate estimate for determining how much change, if any, there had been in reproducing the copy circle of 60^{mm} in diameter.

TABLE VII.

A. *Size of successive circles.*

Subject.	Hand used.	Serial number of circle. 1	2	3	4	5
B.	R	62	64	66	63	62
	L	59	62	60	62	60
M. C.	R	64	61	58	57	54
	L	61	58	59	59	62
C.	R	60	61	62	64	68
	L	58	62	61	65	69
H.	R	61	60	58	57	55
	L	62	63	64	63	62
J.	R	61	62	63	65	68
	L	58	59	61	60	60
S. F.	R	61	62	64	65	67
	L	61	62	62	63	64
J. F.	R	63	58	60	60	60
	L	63	63	65	60	63

B. *Daily average size of circle.*

Subject.	Hand used.	Serial number of experiment. 1	2	3	4	5	6
B.	R	65	59	62	63	67	65
	L	57	61	61	62	63	60
M. C.	R	66	62	57	55	55	54
	L	64	61	61	60	57	56
C.	R	61	62	63	63	65	65
	L	61	62	62	64	64	65
H.	R	60	58	58	57	57	56
	L	61	63	63	62	63	63
J.	R	68	65	64	63	63	62
	L	59	63	57	63	61	55
S. F.	R	70	65	65	63	62	60
	L	70	64	61	59	59	58
J. F.	R	62	61	60	60	60	60
	L	66	64	63	62	60	62

The unit of measurement is 1^{mm}.
In section A, each figure is the average of six experiments.
In section B, each figure is the average of five circles.
The probable error of a determination varies from $\pm 2.1^{mm}$ to $\pm 0.3^{mm}$.

There were two points to which the subject was required to direct his attention in making each figure: the roundness of the figure and its correspondence in size to the copy. We shall now answer the question whether the subject directed his attention more especially to the former or the latter point.

The results given in the above table show three types of practice: (1) that in which the subject decreased the size of the circle both during the progress of each experiment and from day to day, (2) that in which the size of the circle was increased during the experiment but decreased from day to day, (3) that in which there was but little variation either during the experiment or from day to day. The first two classes characterize those who regarded more carefully the smoothness of contour of their own drawn curves than they did the correspondence in size to that of the copy. The third class were those who directed their attention more especially to the size of the curve, and who observed the copy closely each time before beginning to draw their own curves.

The measurements for B. showed that during the first three experiments he began by making the circle smaller than the copy, but increased its size during the experiment until it was larger than the copy, whether the right or the left hand was used. During this period he made the circles by moving each hand counter-clockwise. During the practice of the last three experiments, for some unknown reason, he reversed the direction of the movement for each hand and as a result the size of the successive circles was constantly decreased. The subject was not conscious of having made any change in the direction of the movement of either hand, and was unable to say which way he had moved his hand in the last experiment until he had made the motion as if making a circle, so unconsciously was the movement performed. Questioning the subject at the close of the series brought out the fact that this reversal may have been due to practice on the inverted oval as seen in the capital *W*. Excepting the change in the size of successive circles with the change in the direction of the movement of the hand, all the changes occurring during any one practice may be easily accounted for. If, for instance, after making the first circle, he judged that one to have been smaller or larger than the copy, then in the following he proceeded to correct it.

The figures given in Table VII. show that he made greater effort to approach the copy when practicing with the left hand than he did with the right. Moreover, I observed that he regarded the copy more carefully each time when practicing with the left hand. The care directed toward accuracy of contour and size of circles drawn with the left hand caused the time consumed in making the circles with the left hand to be about one-fourth longer than with the right hand.

With M. C., the right hand was directed counter-clockwise, as was the case with all the other subjects save B., already mentioned; the left was moved in the opposite direction. Starting with a diameter of 71^{mm} for the first circle with the right hand, she reduced the diameter to 61^{mm} during the first experiment. Likewise the decrease in the size of successive circles continued throughout the series of experiments, becoming less, however, at each successive experiment. The average of all the experiments, Table VII., shows a decrease of 10^{mm} in the size of successive circles; and in the daily averages there was a total decrease of 12^{mm}. These figures show that the subject decreased the size of the circle with the right hand both during the process of the experiment and from day to day. Likewise the average of successive circles for the left hand shows that the size of the circle was decreased in the second circle, but increased thereafter. The decrease of the size of successive circles for the left hand was not so large as for the right, which indicates that the subject directed her attention more especially to the size of the circle when practicing with the left hand, and to the smoothness of contour of her own curves when practicing with the right.

The subject C. seemed to give closer attention to the copy when practicing with the left hand but more especially at the beginning of eachexperiment. As was the case with most of the others, the principal consideration at the beginning of each experiment seemed to be to make the circle the same size as that of the copy; but when the copy did not correspond to the natural adjustments of the muscles and when an attempt to follow the size of the copy caused an increase of the error, the subject's æsthetic feeling for smoothness of contour in his own curves soon gained control of the movements, and consequently the size of the copy was wholly disregarded.

Since S. F. made such small gains in the decrease of the angularities of her curves, we are justified in concluding that she directed her attention more especially to making her curves the same in size as the copy. Though there was a constant tendency to increase the size of successive circles throughout the experiment, yet the table shows that she decreased the average size of the circles in six days' practice with the right hand from 70^{mm} to 60^{mm}, and with the left from 70^{mm} to 58^{mm}. I observed that during the first experiment she paid more attention to the smoothness of her curves than to their size, but with practice, however, she became more regardful of their size. In order to make a test on the point whether the size of the copy had any influence on the error in the drawings, I made a few experiments on this subject in which no copy was used. As a result the circles were somewhat larger than those made at

the first experiment and the error was less. These results for S. F. furnish an example of the development of the inhibitory powers in overcoming the muscular adjustments for larger movements of the arm.

The most important feature of practice with J. F. was the approach in the size of the circles to that of the copy. Questioning the subject at the close of the series brought out the fact that she had fixed her attention more especially on the size of the circle. Her statement is confirmed by the large number of alternations in successive circles, so numerous in fact, that there were scarcely two successive circles of the same size. There was, moreover, no tendency to increase or decrease the size of successive circles as with the other subjects. The correcting process was continued throughout the series but the corrections decreased in amount as practice continued day by day. For instance, there was a variation of 8 ^{m} in the size of the circles drawn with the right hand on the first day while there was only 3^{mm} variation on the last day. Such a gradual reduction of the variations in the size of successive circles from day to day demonstrates the influence of practice in giving control over muscular adjustments. The exaggerated adjustments are not in my opinion so much due to the lack of judgment as to the inertia of the muscles; when they do yield to the will, such momentum is acquired that the movement is as likely as not to be exaggerated. The great difference in the development of control over muscular adjustments between J. F. and the other subjects was due to the close attention that she gave to the copy before attempting her circle. She not only observed the copy carefully at the beginning of the exercise but also before attempting each circle.

Daily decrease of error expressed in percentages.

The percentages given in Table VIII. were computed according to the method given on page 61; they show the comparative effects of practice on the different subjects. We notice that equal amounts of practice had influences on the different subjects ranging in value from — 12 % to + 37 %. In the case of B. the average gain was 8 % for the right hand and 13 % for the left. This signifies that although losses occurred on some days yet upon the whole, the good effects of practice overbalanced the evil effects of practice.

The percentages for M.C. show a decrease from day to day with each hand. It is particularly noticeable in the case of the left hand where the gain of the last over the first day amounted to 50 %, which, however, decreased very rapidly until the sixth day when there was no gain over the preceding day. These percentages show that practice had 3 % greater effect in the development of control over the movements of the

left hand than for the right. On the contrary, with C. the gain was 12 % larger for the right hand than for the left. Moreover, the percentages from day to day showed no diminution in the influence of practice.

TABLE VIII.

Relative average daily decrease of error due to practice.

Subject.	Hand used.	Serial number of day. 1	2	3	4	5	Ave.
B.	*R*	0.18	—0.11	0.22	0.23	—0.12	0.08
	L	.60	.31	.14	— .30	— .32	.13
M. C.	*R*	.32	.31	.14	.21	.17	.23
	L	.50	.42	.24	.17	.00	.27
C.	*R*	.27	.33	.27	.12	.25	.25
	L	.16	.08	.11	.17	.12	.13
J. F.	*R*	.30	.37	.33	.50	.33	.37
	L	.29	.27	.26	.25	.50	.32
S. F.	*R*	.17	.21	.28	.20	.00	.17
	L	.31	.25	.13	.20	.00	.18
H.	*R*	.18	.22	.17	.07	.14	.15
	L	— .23	— .15	.10	— .19	— .11	— .12
J.	*R*	.58	.29	.20	.30	.25	.32
	L	— .02	.36	— .04	.46	.14	.18

The percentages given in this table were computed from the results given in Table V. according to the formulas explained on page 61. The values in columns 1, 2, ..., 5, are calculated by formula (2), those in the last column by (3).

The very considerable effect of practice on J. F. showed itself both in the increase of the percentage from day to day and in her making a larger average percentage of gain with each hand than any of the other subjects. As already observed, S. F. varied from the copy more with the left hand than with the right, the reduction of the error being 1 % greater for the left hand than for the right. Considering the fact that she was decidedly right-handed, this is important as it shows how a change in the fixation of attention alters the influence of practice. On the other hand, with H. we find that practice had an opposite effect, for with the right hand there was a gain of 15 %, while with the left there was a loss of 12 %.

Error for successive circles expressed in percentages.

In the same manner as in the preceding section, the percentage of decrease in the error for successive circles was determined, which showed in every case a gain with the right hand during each practice period, ranging in values from 1 % to 27 %. With the left hand, the percentages varied from — 20 % to 21 %, though in four out of seven cases,

the percentages showed a loss for the left hand. This demonstrates very clearly that with untrained muscles a constant standard can usually be sustained only for a very short interval of time. However, the ability for continuing the exercise with increased regularity for a longer time increases with practice.

TABLE IX.

Relative average decrease of error for successive circles.

Subject.	Hand used.	Serial number of circle. 1	2	3	4	Ave.
B.	R	14	7	—6	—11	1
	L	6	—17	11	—29	—20
M. C.	R	22	17	6	0	11
	L	4	—17	—15	—14	—10
C.	R	21	7	10	20	19
	L	17	11	17	12	14
J. F.	R	29	20	30	25	26
	L	14	18	12	0	11
S. F.	R	21	27	20	40	27
	L	28	22	33	0	21
H.	R	19	9	14	0	10
	L	6	—8	—30	—11	—12
J.	R	25	17	25	20	22
	L	—54	—4	10	—25	—18

The percentages given in this table were computed from the results given in Table VI. according to the formulas on page 61.

Observations and deductions.

I observed that when a special effort was made it was usually accompanied by unnecessary movements of the body. For instance, C. in the first part of the series of experiments would contract the jaw muscles; J. would bite his lips; some would twist the mouth; others would knit the eyebrows. As practice continued, however, and the action became more habitual, these distortions for the most part disappeared. Undoubtedly whenever there is a tension of some muscles while others are being vigorously exercised, they become influenced in proportion to this tension.

In some instances I have observed that when the subject noticed an irregularity in his figure, a desire to improve upon this seemed to excite the nervous centers so much that the following effort would not be so good as the preceding. Although the gain in proficiency is not entirely a physiological process, yet any chain of actions must be repeated a number of times before it becomes established in the automatic centers. However, the amount of practice required for any chain of actions to be

carried on with the minimum amount of attention and effort is a psychological factor, depending upon the intellectual vigor of the individual.

These experiments bring out some striking differences in the development of the movements of the right and left hands. For instance, the practice curve for the right hand followed closely the direction of the habit curve; while with the left, instead of there being a gain during each practice, there was, in some instances, a decrease in accuracy of the drawings as the practice continued. This difference in the practice curves for each hand, I attribute to the special effort of attention called forth at the beginning of the experiment with practice of the left hand. When practice is carried on until the movements become irregular, the practice becomes injurious, for the irregular movements become incorporated into the chain of reactions as certainly as do those which are purposefully directed. Therefore, practice may tend to establish irregular adjustments as well as regular ones. Speaking figuratively, the capital on hand at the beginning of each succeeding practice period is the sum of the preceding practices. Consequently, the larger the probable error of the average of all the preceding practices, the more irregular will be the movements of the muscles at the succeeding practice. Hence better results might have been obtained in those cases where the error increased after the third circle if the practice periods had been shorter in the first part of the series and more prolonged in the latter part.

Owing to the fact that some of the subjects increased the size of successive circles and others decreased them, we are justified in concluding that there is a certain adjustment of the muscles in writing and drawing most suitable for each individual which should be taken into consideration when training the muscles for accurate adjustment. If the size of the copy corresponds to the natural adjustment, the subject needs only to direct his attention to the smoothness of his figures, otherwise, he has to contend with the distracting element of the size. Therefore the amount of gain in accuracy of adjustment will be influenced thereby. The general conclusion is that, in the earlier stages of muscular development, the size of the copy should be adjusted to the natural movements of the muscles. If this is not done it may prove such a distracting element that the subject will discard it altogether, for, as we have seen, the attention is always directed first toward smoothness of contour, or freedom from angularities. Psychologically the order of development is in such movements as writing and drawing, (1) reduction of irregularities, (2) correctness in size; and in movements where agility is involved, (1) rapidity, (2) regularity.

The correspondence in the decrease or increase of the size of the

circle and the average daily error, as shown in these experiments, indicates (1) that some distinct relation exists between the error and the size of the circle; (2) that the subject's attention, especially in the case of the right hand, was usually directed to the decrease of the error rather than to making the circles of a size corresponding to that of the copy; (3) that the muscles of the right hand, trained to make certain movements, found it difficult in some instances to establish an entirely new set of reflexes.

Finally, these results support the principle that a short exercise often repeated is the best method of practice for rapid development of accurate adjustments of the muscles. There is no doubt that many of the long exercises in writing and drawing and other subjects in the school-room often engender habitual inattention in the pupils. They are often compelled to write continuously for several minutes; the wisdom of this is doubtful when we consider that in the case of well-developed persons five trials at one time in the experiments with circles gave the best general results attainable at one sitting. Hence long practice at writing, drawing, piano-playing, etc., seems to be time and energy wasted, for not only are inattentive habits cultivated, but every wrong adjustment of the muscles gains a place in the chain of subconscious memories and therefore delays the development of the control over the muscles for accurate adjustments. The practice at each sitting should last only so long as the movements are purposefully directed.

III. Development of control over untrained muscles.

The object of this set of experiments was to ascertain the influence of practice on entirely untrained muscles and less adaptable joints. The experiments, made on the left large toe of Mr. Davis, covered a period of ten days. Four phases of the toe's movement were recorded, the time of the downward motion, the downward rest, the upward motion, and finally the upward rest. As each movement of the digit required the exercise of several muscles, the object of measuring each of these four phases of the toe's movement was to show: (1) the differences in time for the phases of the movement; (2) the influence of practice on the phases; (3) the influence of practice on the shortening of the time of the entire movement of the toe.

Apparatus.

The apparatus was virtually the same as that previously described in these Studies.[1] The single 100 v. d. fork was, however, replaced by the

[1] Scripture, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 113.

PFEIL and DEPREZ markers from which wires led into the quiet room[1] and were there connected with a double-contact key. The markers wrote directly on the smoked surface of the drum; the points were placed parallel to each other and in a line tangent to the surface of the drum. When the drum was revolved, the distance between the marks caused by movements of the armatures could be measured with great accuracy.

The key knob moved through a distance of 5^{mm}. At the beginning of the downward movement of the toe the back contact was broken; at the end of the downward movement the front contact was made; at the beginning of the upward movement, the front contact was broken; at the end of the upward movement the back contact was made. Thus the limits were marked for the four phases, downward movement, lower rest, upward movement, upward rest. The latent times were compensated.

Daily averages of the tapping time.

The average tapping times for successive days are given in Table X. The series of taps during each experiment was divided up into three parts. The first section in Table X. includes the averages of the first 30 taps; the second, the 31st to the 50th tap; and the third, the 51st to the close of the experiment.

The results show an almost constant increase in speed from day to day. Beginning with an average of 436^{σ} on the first day, the average tapping time became 212^{σ} after ten days' practice. Only on one day was there a loss and then the subject was indisposed.

Fatigue was always present after about the 50th tap, appearing sooner on some days than on others, as indicated by the probable errors in section C. of Table X.

Practice generally has its greatest effect between the 30th and 51st taps. The first 30 taps formed the training period of the muscles in which the tapping was constantly accelerated. When the exercise was continued until the muscles were partially overcome by fatigue, the tapping became slower and much more irregular. The portion of the experiment lying between the 30th and 51st taps was chosen arbitrarily as a uniform measure for comparison. The most regular part of the day's practice corresponded closely to that marked off in these boundaries. The tendency was, however, for this zone of regularity to move each day slightly farther away from the beginning.

Of the different phases of the toe's movement, we notice that the movement upward was longer than the downward one; likewise the upward rest was longer than the downward rest. The tension of the spring

[1] See above, p. 52.

of the key, although made too slight to be considered, may have also contributed to make the downward rest shorter. That the touch stimulus was the main factor appears likely because by practice the upward rest tended to decrease more rapidly than the downward rest.

TABLE X.

Daily average of tapping times.

Date, 1898.	Movement downward.						Downward rest.						Movement upward.					
	A	p.e.	B	p.e.	C	p.e.	A	p.e.	B	p.e.	C	p.e.	A	p.e.	B	p.e.	C	p.e.
XII–14	45	14	39	6	47	12	56	13	97	40	62	20	88	38	84	40	59	18
" 15	31	3	23	7	45	14	63	16	57	14	116	41	40	12	27	9	61	18
" 16	31	5	41	11	42	12	87	41	69	28	77	33	40	9	38	9	41	8
" 17	30	5	32	5	41	12	56	10	56	15	71	17	31	9	31	5	44	12
" 18	29	6	35	10	32	7	86	33	80	25	107	40	30	6	33	4	43	19
" 19	34	9	32	5	34	8	57	13	66	21	63	19	37	10	36	4	46	14
" 20	46	13	58	21	45	11	66	19	70	13	77	16	41	13	38	14	48	10
" 21	29	3	30	3	37	9	73	25	69	9	77	25	27	3	28	3	35	6
" 22	36	10	33	8	37	9	74	19	75	10	83	17	36	9	26	4	30	8
" 23	29	3	31	6	26	4	64	26	74	18	75	13	24	3	25	4	25	4

Upward rest.						Entire movement.		
A	p.e.	B	p.e.	C	p.e.	Tap-time	p.e. in σ	p.e. as %
267	94	272	98	231	70	436	103	24
153	56	169	64	175	67	322	73	23
138	60	98	40	147	69	296	74	25
171	99	150	62	135	52	284	68	24
112	44	102	45	134	54	281	61	22
109	49	138	58	153	65	273	62	23
157	62	97	31	109	45	275	68	25
105	43	93	37	120	53	241	54	22
81	22	76	25	83	21	221	36	16
99	29	73	22	71	18	212	35	17

Unit of measurement, $1^\sigma = 0.001^s$.

The probable error of a determination varies from $\pm 23^\sigma$ to $\pm 0.5^\sigma$.

A, the daily average of the first 30 taps.

B, " " " " " following 20 taps.

C, " " " " " remainder.

p.e., probable error.

The most noticeable effect of practice consisted in the change of the probable error of the upward rest, which decreased after ten days' practice from 94^σ to 29^σ for the first 30 taps; for next 20 taps from 98^σ to 22^σ; and after first 50 taps, from 70^σ to 18^σ. Likewise, the movement upward shows a greater gain in regularity than the movement downward; it decreased in section A., from 38^σ to 3^σ; in section B., from 40^σ to 4^σ; and in section C., from 18^σ to 4^σ. The conclusion to be drawn

from the disparity in the amount of decrease of the probable error for the different phases of the toe's movement is that, since in the movement upward and in the upward rest there was a greater amount of voluntary

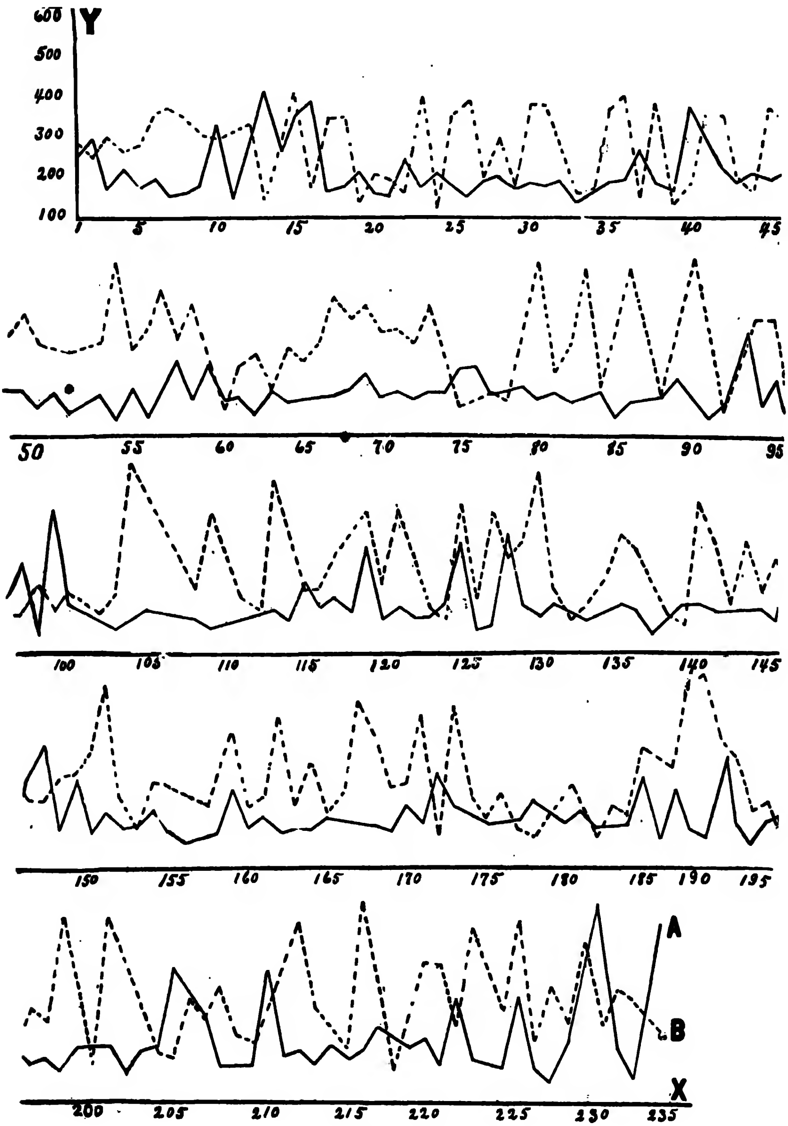


FIG. 10.

X, serial number of tap.
Y, tap time in thousandths of a second.

effort, they were more irregular in the beginning; and as the movement came to be controlled by the automatic centers, the influence of practice was more manifest in these two phases of the toe's movement. Consequently we conclude that in beating time that phase of the movement which causes the greatest irregularity consists in the change of the motion upward to the motion downward.

The daily average tapping time, Table X., shows a decrease of the probable error from 103^{σ} to 35^{σ}. Moreover, when the error is expressed as a percentage of the tapping time, it shows that the increase in regularity was larger than the gain in speed, for the relative gain decreased from 24% to 17%.

The average daily increase in speed, according to the formula given on page 61, was determined, with the following results: 169, 62, 40, 33, 37, 39, 50, 25, 9^{σ}. This expressed as percentages gives: 39, 19, 14, 12, 13, 14, 18, 10, 5%. These figures show that the first day's practice resulted in the largest gain, which, however, rapidly decreased as the practice was continued from day to day. They moreover show that in simple movements, as in tapping, the effect of practice is greater in proportion to the undeveloped state of the muscles.

Physiological effects of practice.

The differences in the fluctuations in the curves of Fig. 10 show the influence of practice. The A curve is the practice curve of the last experiment; B, that of the second experiment.

These curves show how the periods of slight paralysis varied during the two experiments. In curve B the variations follow each other in rapid succession. In curve A they are not so frequent and the recovery is much more rapid. The fluctuations, though larger throughout the series of curve B, increase in amplitude and frequency toward the close of the experiment. In curve A the fluctuations are not so pronounced in the second half even as in the first half. Hence the most prominent physiological effect of practice is to delay the arrival of entire paralysis of the muscles and the reduction of fluctuations in the practice curve. This depends, however, to a large extent on the nervous condition of the subject, for on some of the intervening days absolute paralysis of the muscles occurred before the 125th tap.

At the beginning of the series of experiments intense pain was felt in the calf of the leg after a short period of practice. As exercise continued, the pain became sympathetically induced also in the calf of the other leg.

The coming on of fatigue was characterized by the following stages: (1) a feeling of strain throughout the tendon above the heel; (2) slight

pain in the calf of the left, or exercised, leg; (3) pain was felt in the muscles controlling the upward flexion of the toe; (4) paralysis gradually spread over the whole of that leg while the pain continued to increase in the above mentioned muscles; (5) the pain was finally felt in the calf of the right leg. On the fourth day the pain did not recur in the calf of the leg but was felt in the tendon immediately over the second joint of the toe; but before the close of the experiments it had ceased altogether.

Likewise, in a series of dumbbell exercises wherein I acted as subject, on first to fourth days inclusive the pain was so intense that I was able to exercise only a short time, but after the fourth day I was able to raise one dumbbell over 1000 times without experiencing any actual pain, the only noticeable effect being considerable fatigue.

Throughout all the experiments, I noticed that on some days there was a tendency to decrease the speed from the very start, while on others there was at first a period of acceleration for a limited time before the decrease began. I consequently performed some experiments upon myself in order to determine whether this was connected with the rate of the heart-beat. The experiments were made in tapping with the toe and in the exercises with the dumbbells. Although the tests were not entirely conclusive yet in a general way I found that there was at least some relation between them. When the movements were as rapid as possible there was a decrease in the heart's action. I also observed that when the nervous system was in a relaxed state there was a rapid increase in the pulse beat in the early part of the exercise, the pulse becoming full and firm. Accompanying this there was an increase in the rate of voluntary activity.

The fluctuations in the practice curves may be due to the following causes: (1) fluctuations of the attention caused simply by the lack of effort on the part of the subject; (2) local paralysis of the centers governing the muscles brought on by exercise; (3) mental paralytic strokes causing fluctuations which are generally ascribed to mere fluctuations of attention; (4) general physiological fatigue of the whole body; (5) general mental fatigue.

The above conditions are ever changing, for the physiological effects of practice and exercise depend upon the supply of energizing material stored up in the muscular tissue, the supply of oxygen to the blood, temperature, etc. The mental condition is, moreover, more or less dependent on the physiological condition.

General mental fatigue was distinguished from general muscular fatigue by the feeling that resulted after a period of violent exercise. The muscles of the subject sometimes became fatigued almost to the de-

gree of paralysis, but no special mental effects were felt. On one day after the subject had been engaged with some difficult computations, the change of exercise seemed at first to be a relief, but in a few seconds it became quite difficult for him to control his attention. Though the rate of tapping on that day was as rapid as on any preceding day, yet the mental fatigue was much greater.

A distinction can be made between mental and muscular fatigue by the fact that the coming on of mental fatigue is spasmodic and irregular. An aberration of the attention marks the initial stage of mental fatigue. This soon passes over into a stage which materially effects the regularity of the muscular action.[1] No such irregularities characterize muscular fatigue which is governed by physical laws controlling the breaking down of muscular tissue and the dissemination of waste products.[2] Mental fatigue, however, is subject to the effects resulting from fixation of the attention and thus partakes of all its fluctuations and irregularities.

Effects of practice on muscular action.

1. Practice causes increased circulation, furnishing thereby a large supply of food materials to the muscles, and more rapid dissemination of waste products. Thus the muscles are able to store up energy and give it out more readily on demand. But the storing of energy is not all. For example, gymnastic exercise has a higher purpose in view than to bring the body merely to that state of perfection found in the case of the common laborer. The development of muscular tissue must be supplemented by an education of the nervous centers in order that they may respond precisely to acts of will. Indeed, the increase of muscular tissue may be looked upon as merely an accidental accompaniment to that mental process which begins by constant fixation of the attention and ends, even where the achievement is most complicated, in automatic and subconscious control of the muscular movements.

2. The time before the coming on of mental and muscular fatigue depends upon the amount of muscular energy and upon the concentration of the attention, both of which are greatly influenced by practice.

3. When for any set of actions the development of the centers has reached an automatic condition, the maximum rate of either mental or muscular rapidity is not reached in the first few seconds of the exercise because a chain of actions cannot be remembered by any act of will but requires the exercise of the muscles themselves to reëstablish the chain of subconscious reflexes.

---

[1] MOORE, *Studies of fatigue*, Stud. Yale Psych. Lab., 1895 III 89.

[2] LOMBARD, in the American Text-book of Physiology, III, Philadelphia 1897.

4. The duration of maximum rapidity is dependent upon the power of the mind for sustained attention. As soon as the attention is diverted the movement comes into the control of the automatic centers. The speed is consequently decreased, for only by a special effort is speed, either muscular or mental, increased above the limit acquired by habit. The utmost speed can be maintained only for a few seconds at a time at first, but the period may be lengthened by practice. Hence the fluctuations in the practice curve are generally due to slight mental fatigue. Although the subject recovers very rapidly at first, yet as the exercise continues the fluctuations recur more frequently and the periods of recovery are lengthened.

5. Another very important element connected with the duration of one's ability for continuing the exercise, is his knowledge of the time that the exercise is to last. If the exercise is to last only a short time, greater effort will be put forth in that period than when the exercise is to continue an hour or more. In long periods of exercise the subject will unconsciously measure out the energy in proportion to the duration of the practice period. For instance, in the experiments made by OEHRN[1] in memorizing syllables, in making successive additions, and in counting letters in groups of threes wherein the exercise was continued from one to two hours, the maximum was reached only in the first instance after 24 minutes, in the second after 28, and in the third after 59. Contrasted with our experiments, his results show that the maximum point depends upon the effort put forth in the beginning of the experiment. The results that I obtained point to the fact that if OEHRN had shortened the practice period, the maximum point would not only have been reached in about one-tenth of the time, but the progress no doubt would have been greater.

Nor does the maximum rate of voluntary movement depend upon an innate sense of rhythm as SCHAEFER[2] implies but is, as CAMERER[3] states, one of constant acceleration until the setting in of mental fatigue. Instead of a rhythmical fluctuation in the voluntary effort, maintained by some investigators, I found no remarkable regularity and am led to consider it as dependent upon several psycho-physiological processes too complicated to have a regular period of oscillation.

6. Practice does not always mean an absolute gain in efficiency; it may

---

[1] OEHRN, *Experimentelle Studien zur Individualpsychologie*, Psychologische Arbeiten (Kraepelin), 1896 I 92.

[2] SCHAEFER, CANNEY AND TUNSTALL, *On the rhythm of muscular response to volitional impulses in man*, Jour. Physiol., 1886 VII 96.

[3] CAMERER, *Versuche über den zeitlichen Verlauf d. Willensbewegung*, 41–45, Tübingen 1866.

even produce negative results. It is generally taken to signify improvement; but observation and the results of experiments discussed in preceding pages, show that, when either muscular rapidity or regulated movement is required, practice may be even detrimental to development. Every action leaves its trace on the nervous matter; every effort put forth tends to establish itself so that acts immediately succeeding it follow as a matter of inertia. If the effort put forth is small and the action slow, habit then establishes itself for that mode of action. The law of the growth into habitual automatic control takes into account every activity. If a higher speed of activity or a regulated movement or a certain readiness in mental grasp is desired, then every thoughtless action tends to establish itself and delays attainment to the desired standard of efficiency.

It requires the same effort to overcome the condition occasioned by bad effects of practice as it does to establish a new mode of functioning. Hence energy is wasted when the practice is not thoughtfully directed. Therefore, we conclude that not only is that part of practice efficient for growth in regulated movement, in speed of muscular adjustment, or in quickness of mental grasp, which is accompanied by conscious effort, but the unconscious adjustments also have their effects and should be directed properly.

7. The feeling of satisfaction or of having attained one's limit is another not less important element in the development of rapidity in either mental or muscular activity. Every advancement either in mental quickness[1] or muscular activity requires a certain effort, depending on the stage of development already attained. The greater the speed and the smaller the probable error, the less the gain becomes for the same expenditure of energy. As this developed state is approached, a person feels that his efforts are not sufficiently rewarded, and finally there comes a period when he feels that he has reached the limit of his development. In fact, this constitutes the difference between the novice and the expert. The "plateaus" mentioned by BRYAN[2] in the habit curve would seem rather to indicate resting periods in the effort. If the subject can be induced to sustain the same effort day by day, there would not be any "plateaus" in the habit curve.

If this law be expressed by the general equation $y = f(x)$, where y indicates the amount of gain by practice, we must regard x as containing constant elements of (1) time, (2) complexity of

---

[1] AMBERG, *Ueber den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit*, Psychologische Arbeiten (Kraepelin), 1896 I 30.

[2] BRYAN, *Studies in the physiology and psychology of telegraphic language*, Psych. Review, 1897 IV 27.

the movements, (3) the number of muscles undergoing training, and (4) the growth of automatic control. The last may be resolved into various personal factors such as mental grasp, endurance for sustained effort, and the vividness of the impression. According to this principle, then, the growth of intellectual habits should be more rapid with those who possess the strongest intellectual powers, since, by their power of holding the attention, they succeed in getting the same impression repeated oftener in the chain of reactions. If with intellectual power there is combined strong individuality, or perhaps more properly speaking a strong will, such persons possess superior ability after breaking down one habit and reforming another.

IV. Estimation of time.

The apparatus consisted of a kymograph, to which was attached the Wundt time-sense apparatus with the Meumann star-contacts by means of which adjustments could be made so as to give any interval of time desired. The arrangement of the apparatus was the same as that described in these Studies.[1] The sound of the 100 v. d. fork was conveyed to the subject in the quiet room by means of a telephone receiver. The sound to be estimated came first. After an instantaneous interruption the sound began again. When the subject thought that it had lasted as long after the interruption as before it, he pressed a key which recorded a spark on the drum of the kymograph. The contacts were so

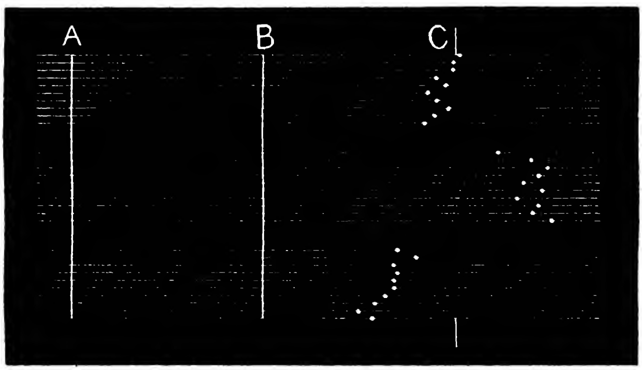


Fig. 11.

arranged that an experiment occupied the first part of every period of 18^s, the remainder of the period serving for rest. This was intended to avoid as far as possible any influence of fatigue. A series of such records is given

[1] Scripture, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 127.

in Fig. 11. *A* represents the beginning of the tone; *B*, the point where it was interrupted; and *C*, the point where the interval of time from *B* to *C* was equal to that from *A* to *B*. The dots represent the first ten estimates of 100^{σ} by the subjects A.F., R.E. and H.J. on the first day; the group at the top of the figure being the estimates for A.F., the second those for R.E. and that at the bottom those for H.J. This may be said to represent in a general way the ability of different individuals to judge intervals of time without practice.

A number of preliminary experiments demonstrated the fact already established by a number of investigators that the time-judgment varied considerably for the same interval with different individuals and with the same individual for different intervals. Attempts have been made to establish a definite interval as that which can be estimated with the smallest amount of error, but the point has varied with each investigator.

The three intervals, 82^{σ}, 100^{σ} and 164^{σ}, were selected because they covered the field of most accurate estimates according to the results obtained by previous investigators.

Those who served as subjects for the experiments were W.J. (Jump), R.E. (Evans), E.F. (Furguson) and B.B. (Brown), students in the Theological Seminary; A.F. (Fisher) and C.S. (Smith), steward and mechanic of the Psychological Laboratory.

Daily average estimates.

The average estimates for successive days are given in Table XI., and are graphically represented in Figs. 12, 13 and 14.

Every precaution was taken to prevent the subjects from counting or moving any part of the body by which they might measure off the time through muscular energy, the object being to ascertain whether a person has an actual time-sense regardless of any form of muscular activity or mental calculation.

An inspection of the results for A. F. shows that there was a constant decrease in the time estimate from day to day. Beginning with 88^{σ} on the first day, he gradually reduced the estimate during 16 days' practice to 55^{σ}, which is but little more than one-half of the time to be estimated. The same facts were brought out in the case of R. E., whose average estimate of 100^{σ} was on the first day 159^{σ}, and 102^{σ} on the eighth day. This shows a decrease of about one-half in the average estimate. H. J. increased his average for the first day, 99^{σ}, until the third day when he reached his maximum point, 116^{σ}, after which the estimate decreased until next to the last day. The very large estimate, 137^{σ}, on the last day, no doubt, was due to nervousness. The record for this day should not be

TABLE XI.

Average estimates on successive days.

	A. F.				R. E.			H. J.		
Date 1898.	Estimate on 100^{Σ}	p. e.	p. e. as %	Date 1898.	Estimate on 100^{Σ}	p. e.	p. e. as %	Estimate on 100^{Σ}	p. e.	p. e. as %
XI–26	88	1.4	1.7	XII–15	159	1.5	0.9	99	1.4	1.4
27	91	1.7	1.9	16	130	1.6	1.2	103	1.6	1.5
28	73	1.1	1.5	17	138	1.8	1.3	116	1.1	1.0
30	88	1.4	1.7	18	118	1.3	1.1	114	1.1	1.0
XII–1	74	1.6	2.2	19	114	1.1	1.0	114	1.5	1.3
2	73	1.5	2.0	20	117	1.1	1.0	109	1.2	1.1
3	58	1.3	2.2	21	107	0.9	0.8	103	1.0	1.0
4	54	1.7	3.1	22	102	0.8	0.8	137	2.3	1.7
5	53	1.2	2.3							
6	58	0.8	1.4							
7	61	1.0	1.6							
8	53	1.3	2.5							
9	55	0.8	1.5							
10	58	1.1	1.9							
11	55	1.0	1.8							
12	55	1.2	2.2							

	B. B.			E. F.			A. F.			C. S.		
Date 1898.	Estimate on 82^{Σ}	p. e.	p. e. as %	Estimate on 82^{Σ}	p. e.	p. e. as %	Estimate on 164^{Σ}	p. e.	p. e. as %	Estimate on 164^{Σ}	p. e.	p. e. as %
I–26	97	1.1	1.1	73	1.5	2.0	91	6.5	7.1	147	3.3	2.2
27	99	1.1	1.1	70	1.0	1.4	99	2.3	2.3	157	2.0	1.3
28	102	1.8	1.8	69	0.9	1.4	105	2.5	2.4	164	2.6	1.6
29	113	1.7	1.5	82	1.5	1.8	119	2.2	1.8	162	2.1	1.3
31	112	1.6	1.4	80	1.1	1.4	118	2.5	2.1	167	2.2	1.3
II– 1	113	1.5	1.3	82	0.9	1.1	157	2.6	1.6	166	3.9	2.3
2	115	0.8	0.7	80	1.5	1.9	154	2.4	1.6	180	2.5	1.3
3	107	1.5	1.4	92	1.8	2.0	155	3.4	2.2	207	2.8	1.4
4	101	1.3	1.3	99	1.2	1.2	169	2.8	1.6	175	2.7	1.5
5	116	1.3	1.1	99	1.4	1.4	150	3.1	2.1	205	2.7	1.3
6	118	1.5	1.3	87	1.8	2.1	159	3.5	2.2	190	2.4	1.3

Unit of measurement, $1^{\Sigma} = 0.01^{s}$.
Number of estimates on each day, 30 to 50.
The probable error of a determination varies from $\pm 10^{\sigma}$ to $\pm 2^{\sigma}$.
p. e., probable error of each estimate from the average.

considered in making up our deductions to be drawn from the results, for the subject was in a very nervous state when the experiment was made. I included the record in the table only to contrast the difference between the nervous and tranquil frame of mind in respect to the estimate of time.

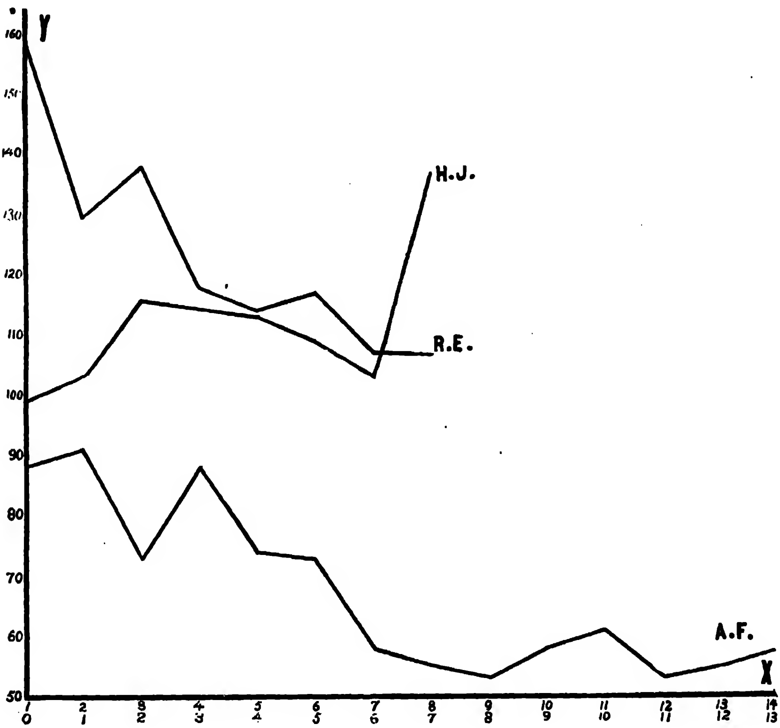


FIG. 12.

X, upper line, serial number of day.
X, lower line, days of previous practice.
Y, estimate of an interval of 100^{σ}.

In the cases of B. B. and E. F., Table XI., where 82^{σ} was the interval given, we notice that with each of these subjects, the time estimate was continually lengthened as practice continued. B. B. began with an average estimate of 97^{σ} on the first day, which was larger than the interval to be estimated and continued to increase the interval throughout the series of experiments. E. F. followed the same course, but he began with an average estimate of 73^{σ} on the first day, which increased daily, until his estimate exceeded the correct amount by 15^{σ}. Hence there was with

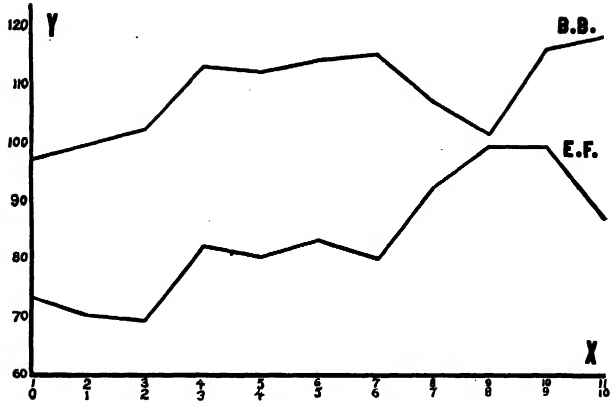


FIG. 13.
X, upper line, serial number of day.
X, lower line, days of previous practice.
Y, estimate of an interval of 82^{Σ}.

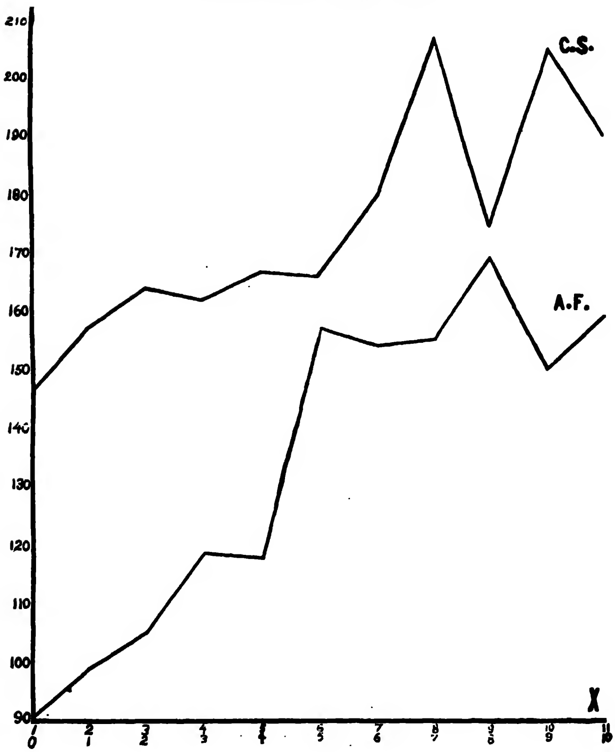


FIG. 14.
X, upper line, serial number of day.
X, lower line, days of previous practice.
Y, estimate of an interval of 164^{Σ}.

E. F. a total increase of 24^s in his estimate, and with B. B. 21^s. These amounts divided by the number of days (11), would give an increase of over 2^s per day. Likewise A. F. decreased his average daily estimate by 33^s in 16 days, a daily decrease of 2^s per day. R. E. decreased his estimate 57^s in eight days, a decrease of 7^s per day. The correcting process present in the case of H. J., no doubt, caused the irregularities in his habit curve, for the subject was aware of the tendency to underestimate long intervals and to overestimate short intervals. Consequently he continually corrected himself. But even in his case, we notice that after the third day's practice there was a tendency to shorten the interval.

When 164^s was estimated, the same facts were found to exist. However, it should be noted that where A. F. decreased his estimate when 100^s was the given interval, he increased it from 91^s to 159^s (the highest point being 169^s, 9th day) in estimating the 164^s interval; C. S., likewise, increased his estimate of 164^s, approximately 4^s per day.

When the 100^s interval was estimated, all may be said to have decreased their estimates of its duration by practice, varying in amount with each individual. Moreover, each had a different conception of its duration in the very beginning of the experiments, ranging from 88^s to 159^s. When 82^s and 164^s were the intervals estimated all the subjects increased their estimates of it by practice. The estimates of 82^s on the first day were 97^s and 73^s. Of 164^s, the estimates were 91^s and 147^s.

In Table XI. are given the probable errors for each day's estimate. In the third column the probable error is expressed as a percentage of the estimate. Though there was a decrease in the absolute probable error with each of the subjects, yet when it is expressed as a percentage there was only one case, C. S., in which the relative probable error decreased more rapidly than the average estimate decreased or increased as the case may be from day to day. This fact demonstrates that the changes in the daily average estimates were not due to fatigue. If the effects of fatigue had been more prominent in the early part of the series than in the latter part then there would have been a decrease in the percentage of error. But there was little cause for fatigue, since the subject was required to make only one judgment every 18 seconds.

The facts brought out in these experiments are important as throwing some light on the various "indifference points" obtained by different investigators. The estimate is a personal factor which varies with each person and with the same person at different periods of practice. To make a test of this point, after the the close of the series I changed the interval of 100^s to 75^s and gave A. F. a few tests. The results were: 50, 49, 38, 49, 49, 59, 36, 43, 47, 39, 53, 35, 39, 33, 49, 51, 38, 43, 33,

36 (ave. 43^{σ}; p. e., 4^{σ}). These figures, however, may have been influenced by practice on the 100^{σ}; the large probable error would seem to indicate it.

The above figures, as well as the changes in the time estimate due to practice, Table XI., would seem to indicate that the interval which we judge with maximum accuracy is a changeable one, depending upon the amount of previous practice. Nor does it seem that our "time-sense," when unaided by some form of muscular action, is sharpened by practice; on the other hand it may become less accurate. Without doubt, if some additional basis were allowed, as in counting or moving of the hand, practice would undoubtedly produce good results, in which instance it becomes a measure of muscular strain when carried into overt acts, or of strain of the attention when only the impulse is felt. The differences then in the time estimate of different individuals depend partly upon their nature, whether impetuous and nervous or temperate and deliberate; partly upon the direction of the attention.

While the nervous temperament may account for the difference in time estimate of different individuals, it does not explain the variations in estimate day by day. For instance, in the case of A. F. (the only subject practiced on two different periods) the case is reversed in the two estimates. He not only underestimated the 100^{σ} at the very beginning of the series of experiments but continued to reduce the estimate throughout the whole series. On the other hand, in estimating 164^{σ} his underestimate was larger in the beginning of the series than at its close. Consequently it may be inferred that the change in the estimates of the two periods was not due so much to the nervous temperament of the person as to the conception of the length of the interval which he entertained at the beginning of the series of experiments.

Summary.

1. The estimate of a given interval varies for different individuals both with and without practice.

2. The estimate varies for different intervals with the same person both with and without practice.

3. Practice on the same interval may cause the variation from the given interval to increase with one person and decrease with another. Or it may cause the variation from the given interval to increase with one interval and decrease with another interval.

4. Time estimate is a personal factor depending upon (a) the nature of the person whether of an impulsive or quiet temperament; (b) upon the point of the fixation of the attention whether to the sensory or to the

motor side; (c) upon the acuteness of the sense of the person to strains accompanying the fixation of the attention and muscular tension.

5. The change that practice produces in the estimation of time is probably due to fixing the attention in the beginning of the series of experiments: (a) to the movement to be performed, in which case the time estimate is shortened in accordance with the growth of automatic control; (b) upon the sensory side in which case the time estimate is made longer; (c) on the idea which the subject entertains of the interval on beginning the experiment—if he considers it very short, he will at first underestimate it and will increase the time estimate by practice; on the other hand, if he considers it very long he may shorten it by practice.

6. Our sense of time may not be sharpened by practice; on the contrary, it may become less accurate.

7. There is no "indifference point" from which the subject does not vary with long continued practice.

8. The overestimation of small intervals may also be due to the fact that the subject does not take into consideration his reaction time. The underestimation of longer intervals may also be due to the fact that the impulse to react keeps ripening and soon reaches such a degree of intensity that the subject cannot withhold the reaction.

V. Regulated rhythmical action.

Apparatus.

By regulated rhythm we understand such actions as beating with a signal. The sound which guided the subjects in beating time was produced by a telegraph sounder, arranged in series with a make contact on a revolving drum. At a point on one of the upright standards supporting the drum a spring was attached. To the drum a small projecting arm was attached. When this arm moved round to the spring attached to the standard supporting the drum, it pressed a lever down, thus closing the circuit through the telegraph sounder, and thereby producing a click in the sounder. Consequently, the time elapsing between successive clicks of the sounder depended upon the speed at which the drum was revolved; the regularity in the frequency of the sounds depended upon the regularity in the speed of the drum.

The drum was run by a small motor to which it was attached by a thread belt. The motor was run by a current produced by three Edison-Lalande batteries, regulated by introducing a resistance of small German-silver wire. In this way the speed of the motor could be adjusted with

great accuracy.[1] This method of producing sounds at regular intervals required a motor and drum of great regularity. A careful investigation was, therefore, made on several kinds of drums and recording arrangements.

1. EDISON *phonograph.* The metal cylinder of the phonograph may frequently be used as a recording drum. Three tests at different speeds were made on the form known as the Home Phonograph. At 267.5^{mm} per second, the probable error was 1.85^{mm}, or 0.7%; at a speed of 174.2^{mm} per second, it was 0.42^{mm}, or 0.2%; at a speed of 354.3^{mm} per second, it was 0.84^{mm}, or 0.2%. The drum is thus a very regular one. The small size of the drum is inconvenient, but the works are so strong that they might be used in running a much larger drum.

2. LUDWIG *kymograph made by* BALTZAR. At the slowest speed, 0.47^{mm} per second, the probable error was 0.008^{mm}, or 2%; at a speed of 0.9^{mm}, the probable error was 0.01^{mm}, or 1.0%; at 28.9^{mm}, the probable error was 0.14^{mm}, or 0.2%; at 254.2^{mm}, probable error was 1.64^{mm}, or 0.6%. I observed that at the slower speeds one is likely to keep the spring wound up too tightly, in which case the probable error is increased. Even one-half turn of the handle which winds the spring will produce considerable variation in its speed.

3. EDISON *motor run by* EDISON-LALANDE *batteries.* Four tests were made at different speeds and on different dates. Three cells, arranged in series, were used to run the motor. A thread from the motor ran the regular recording drum. The first test was made when the cells were fresh. At the high speed of 1037.7^{mm} per second (over two revolutions per second), the probable error was 0.3%.

The next test was made nine days later; the cells had been in constant use in the meantime. The same arrangements were used, but a slower speed was tried. At 159.3^{mm} per second, the probable error was 0.12%. Ten days later another test was made at a speed of 265.5^{mm} per second, the probable error was 0.08%. In another ten days a test was made at a speed of 150.5^{mm}, the probable error was 0.13%.

These tests show that an EDISON motor run by the EDISON-LALANDE batteries properly arranged, is a very regular source of power. It should be stated that the cells must work for several minutes before they become constant. In no case was I able to get a favorable record until the cells had been working at least five minutes. The above tests were made after the cells had been working ten minutes or more.

---

[1] The remainder of the apparatus used was identical with that described in SCRIPTURE, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 121.

4. *Latent time and regularity of the* PFEIL *marker.*

(1) Break contact. When the break contact was used and the magnetic cores were removed as far as possible from the armature to which the metallic point of the marker was attached, the latent time was found to be 1.1^{σ}, with a probable error of 0.09^{σ}. The current was not strong enough to force the armature to react promptly; hence the large probable error. When the cores were placed in close contact with the armature, the remanent magnetism was so strong that it caused the latent time to be considerably lengthened, namely to 14.7^{σ}; but the probable error was reduced to 0.03^{σ}.

(2) Make contact. When the cores were distant, the latent time was 1.8^{σ}, with a probable error of 0.12^{σ}. With the cores close up to the armature, the latent time was reduced to 1.3^{σ}, with a probable error of 0.5^{σ}.

If one desires the smallest probable error possible, then the adjustment with the cores close up to the armature on the make contact, and with them distant on the break contact is the most favorable. Several tests were made on intermediate amplitudes of movement of the armature, with the result that the latent time varied approximately with the distance of the cores from the armature, and the probable error inversely with the distance.

5. *Latent time and regularity of the* DEPREZ *marker.* The results of the test on the make contact showed the latent time to be 2.5^{σ}, with a probable error of 0.64^{σ}. The latent time in the case of the break contact with the same adjustment as above was 3.8^{σ}, with a probable error of 0.07^{σ}. Owing to the delicacy of the instrument, changes in the amplitude of the armature did not affect the latent time so much as they did in the case of the PFEIL marker. It did, however, vary somewhat with the strength of the current passed through it, the latent time increasing with the strength of the current.

6. *Regularity of the spark record.* The probable error of spark records under the usual conditions at the Yale Laboratory is 0.25^{mm}. With the same apparatus and the same kind of paper the above amount is constant whether the drum is still or in motion. Consequently the amount that it will vitiate the record depends upon the speed of the drum. With a fast drum it is negligible; with a slow drum it must be taken into consideration. In these records the sharp metal point of the marker was so bent that it stood perpendicular to the surface of the drum. If it is placed at an acute angle to the surface of the drum, the error is largely increased. When thus placed the spark often-times leaps off at the side rather than at the end of the pointer. The thinnest glazed paper is used.

7. *Noiseless key.* For these experiments a noiseless key was necessary because it was found that, while beating time, the subject would quite as often be guided by the sound produced by the key which he used as by the sound coming from the telegraph sounder. After various attempts to get a noiseless key, that which was found best adapted to the purpose of our experiments consisted of a small band of pendulum-wire soldered to a piece of brass which was fastened in a handle. The band rested against a platinum point so that when it was moved the circuit was interrupted. An insulated wire from one pole of the battery was connected with the platinum point through the handle, while the band was connected with the other pole. A soft substance for the finger to strike against in beating time was fastened over the end of the band. By this method we were able to do away with the guidance which the sound from an ordinary telegraph key gave to the ear in directing the beats. Consequently it was necessary for the subject to direct his attention more to the muscular feeling than to the coincidence or the variance between two sounds as would have been the case had a sounding key been used.

The experiments were made on I. M. (Ishiro Miyake), A. F. (A. Fisher), and W. J. (W. Johnson). The time interval was one second.

Experiments.

A number of preliminary tests on different persons showed a uniform tendency to anticipate the signal made by the telegraph sounder with this form of key, whereas with a sounding key the reverse was quite as often

TABLE XII.

Daily averages.

	I. M		*A. F*		*W. J.*	
Date 1898. Nov.	Constant error.	Probable error.	Constant error.	Probable error.	Constant error.	Probable error.
14	—118	39	—158	44	—159	36
15	—136	35	— 69	37	—148	29
18	— 40	30	— 80	42	—150	22
19	+ 1	66	+ 27	26	— 15	39
20	— 4	29	+ 65	48	— 24	17
21	+ 12	28	— 4	39	— 26	20
22	+ 14	31	+ 10	31	— 37	15

Unit of measurement, $1\sigma = 0.001^s$.

Number of beats at each experiment, 40.

The sign — indicates that the subject beat before the signal was heard; +, that he beat after the signal was heard.

The probable error of a determination varies from $\pm 4\sigma$ to $\pm 9\sigma$.

true. A series of such results with a sounding key has been previously published in these studies.[1]

Likewise the results with the noiseless key given in Table XII. show that all the subjects anticipated the signal, not only on the first day, but also during the first three days in the cases of I. M. and A. F., while W. J. anticipated the signal throughout the series of experiments. In each case the anticipation of the signal decreased until, in the latter part of the series, two of the subjects waited until they heard the signal before beating. The figures given in the table each represent the averages of 40 beats. The constant error was derived by adding all the + and — errors in each experiment, taking their difference and dividing by the number of experiments.[2]

Conclusions.

The probable error for each of the subjects was in the beginning of the series of experiments much less than the constant error. This shows that the tendency of the subject at first was to be governed by the muscular rhythms or his own most natural rate of rhythmical movement. With practice, however, he learned to accommodate the muscular rhythm to the time interval of the signal. However, the diverting of the attention to the signal prevented the rapid decrease of the probable error as was observed in all the other experiments. The psychological order of development in all regulated rhythmical movements is (a) the change from the ordinary rate of muscular action to that of the given rate, and (b) then, the decrease of the probable error.

VI. Free rhythmical action.

The apparatus consisted of a revolving drum on the smoked surface of which the metal point of a Deprez marker wrote. The spark method was used. Wires from a battery of 4 ampères were connected with the mercury cups of a Kronecker interrupter. As the vibrating arm of the interrupter dipped down into the mercury, it closed the circuit through the spark coil. The arm of the interrupter was kept vibrating by a separate self-interrupting circuit through the magnets on either side of the arm. From the secondary coil of the spark coil, one wire led to the drum and the other was connected with the support of the Deprez marker so that at each make of the interrupter there was produced a spark on the line drawn by the point of the marker. The interrupter was adjusted to vibrate 10 times a second.

[1] Scripture, *Elementary course in psychological measurements*, Stud. Yale Psych. Lab., 1896 IV 123.

[2] Scripture, New Psychology, 182, London 1897.

In order to prevent irregularities in the spark record and to keep the mercury from oxidizing, a stream of water was allowed to flow over the surface of the mercury. According to the original plan two flasks, connected by rubber tubing with the mercury cups, were filled with water and placed above at sufficient height to get the necessary amount of pressure. This, however, was replaced by a much more convenient reservoir, arranged by Dr. Scripture, which was fastened to a tripod and placed a little above the interrupter. The reservoir was connected by rubber tubing with a hydrant. Two rubber tubes were led from the base of the reservoir to the two mercury cups connected with the interrupter. Any amount of water could in this way be supplied. In the event of too large a supply there was a waste pipe connected with the reservoir which prevented it from overflowing.

The Deprez marker was connected with a break contact key in the quiet room. Each movement of the key made a break in the circuit, and this in turn produced a movement in the armature of the marker. As in the preceding section, the noiseless key was used because it was desired to ascertain the rhythm of the person when unaided by the ear. The key was held between bags of sand placed on the lap. No restrictions were placed on the amplitude of the movement of the finger or hand in beating time. The subject was directed to choose his own rate of movement and was requested to retain the same speed throughout the series of experiments. All explanations were made before the beginning of the experiments. The subjects were not informed of their rate of movement. In most instances the watch which the subject carried in his pocket was laid aside because it was found that the ticking of the watch caused distraction of the attention with some subjects.

Daily averages.

The figures given in Table XIII. show that all the subjects constantly decreased the interval between beats as practice was continued. Though each practice lasted during the time required to make from 250 to 400 beats, only sections of each practice are given in the table. Section I. gives the averages of the first 40 beats; section II. the averages of the next 40 beats; and section III. that portion included between the 160th and 201st beats.

The shortening in the average time of the beat is apparent in every case. The shortening was probably due to the increasing ease with which the subject performed the experiment. His mental processes probably became more fluent as the result of practice. It was also noticed in several cases that the amplitude of the movements of the hand also de-

TABLE XIII.

Average daily rhythmical movement.

Name.	Date 1898.	1st 40 beats.	p. e.	p. e. as %	P.E.	2d 40 beats.	p. e.	p. e. as %	P.E.	Beats from 160 to 200.	p. e.	p. e. as %	P.E.
	XI—20	73	3.2	4.4	0.5	64	3.4	5.3	0.5	53	1.7	3.2	0.3
	21	53	2.4	4.5	0.4	52	2.0	3.9	0.3	49	2.1	4.3	0.3
	22	46	1.5	3.3	0.2	45	1.2	2.7	0.2	45	1.0	2.3	0.2
A. F.	23	43	1.9	4.4	0.3	42	1.9	4.5	0.3	45	2.0	4.4	0.3
	24	40	1.7	4.2	0.3	37	1.2	3.2	0.2	33	1.0	3.0	0.2
	25	36	1.2	3.3	0.2	35	1.3	3.7	0.2	34	2.2	6.5	0.4
	26	32	1.1	3.4	0.2	32	1.3	4.0	0.2	32	1.1	3.4	0.2
	27	25	1.2	4.8	0.2	25	1.1	4.4	0.2	25	1.1	4.4	0.2
	X—25	99	3.1	3.2	0.6	91	3.0	3.3	0.5	89	3.4	3.8	0.6
	27	73	3.2	4.4	0.5	76	2.9	3.8	0.5	71	4.1	5.8	0.7
	28	69	2.6	3.8	0.4	64	2.7	4.2	0.4	61	2.2	3.6	0.4
W. S.	29	62	1.9	3.1	0.3	60	1.5	2.5	0.3	60	1.7	2.8	0.3
	30	63	2.7	4.3	0.4	60	1.8	3.0	0.3	57	1.9	3.3	0.3
	XI— 1	61	2.2	3.6	0.4	60	1.2	2.0	0.2	56	1.7	3.0	0.3
	2	59	1.5	2.5	0.3	58	1.0	1.7	0.2	55	1.6	2.9	0.3
	X—21	84	3.1	3.7	0.5	86	2.9	3.4	0.5	87	3.3	3.8	0.5
	22	56	1.4	2.5	0.2	55	1.1	2.0	0.2	54	1.6	3.0	0.3
	23	63	1.7	2.7	0.3	65	1.7	2.6	0.3	62	2.0	3.2	0.3
C. S.	25	59	1.7	2.9	0.3	60	1.4	2.3	0.2	58	1.5	2.6	0.3
	26	55	2.4	4.4	0.4	54	2.1	3.9	0.3	54	1.7	3.2	0.3
	27	54	2.1	3.9	0.3	53	1.7	3.2	0.3	51	1.6	3.1	0.3
	28	52	1.7	3.3	0.3	52	1.2	2.3	0.2	51	1.4	2.8	0.2
	X—23	185	4.6	2.5	0.7	175	5.2	3.0	0.9	163	5.1	3.1	0.8
	24	110	3.5	3.2	0.6	108	4.0	3.7	0.7	101	6.2	6.1	0.9
E. F.	25	133	5.5	4.2	0.9	137	3.5	2.5	0.6	143	3.7	2.6	0.6
	26	122	3.5	2.9	0.6	117	2.4	2.1	0.4	123	2.9	2.4	0.5
	27	114	2.5	2.2	0.4	112	2.0	1.8	0.3	108	2.5	2.3	0.4
	X—23	78	3.1	4.0	0.5	75	1.9	2.5	0.3	75	3.5	4.7	0.6
	24	66	2.5	3.9	0.4	64	1.9	3.0	0.3	63	1.6	2.5	0.3
E. W.	25	70	2.3	3.3	0.4	68	1.7	2.5	0.3	63	2.0	3.2	0.3
	26	64	1.8	2.8	0.3	64	1.5	2.4	0.3	60	2.0	3.3	0.3
	27	63	1.6	2.6	0.3	62	1.2	1.9	0.2	59	1.6	2.7	0.3
	XI—23	44	3.0	6.8	0.5	40	1.8	4.5	0.3	37	1.5	4.1	0.2
	24	40	2.0	5.0	0.3	37	1.0	2.7	0.2	33	1.0	3.0	0.2
J. P.	25	40	1.5	3.8	0.3	38	1.2	3.2	0.2	34	1.0	2.9	0.2
	26	37	1.4	3.8	0.2	36	1.0	2.8	0.2	33	1.3	4.0	0.2
	27	37	0.9	2.4	0.2	35	0.8	2.3	0.1	33	1.0	3.0	0.2
	X—31	53	3.0	5.7	0.5	49	3.1	6.3	0.5	46	1.9	4.1	0.3
	XI— 1	56	3.8	6.8	0.6	54	2.5	4.6	0.4	50	2.8	5.6	0.5
	2	54	2.3	4.3	0.4	52	2.0	3.9	0.3	46	1.9	4.0	0.3
W. J.	3	50	2.4	4.8	0.4	49	1.8	3.7	0.3	46	1.9	4.1	0.3
	4	48	1.6	3.4	0.3	48	1.2	2.5	0.2	46	1.5	3.3	0.3
	5	47	1.2	2.6	0.2	47	1.0	2.1	0.2	45	1.6	3.6	0.3
	6	47	1.0	2.1	0.2	46	1.0	2.1	0.2	45	1.2	2.7	0.2

The unit of measurement is $1^{\sigma} = 0.01^{s}$.

The number of experiments at each practice ranged from 250 to 400.

The probable error of a determination varied from $\pm 9^{\sigma}$ to $\pm 2^{\sigma}$.

p. e., probable error of each beat from the average on that day.

P. E., probable error of determination.

creased; this was probably due to the increasing ease of the muscular adjustments.

The results also show for most subjects a decrease in the relative probable error, but for others a nearly constant probable error. This indicates for the former class an increase in regularity. Those who did not improve to any extent in regularity were those who most shortened the average beating-time. It may be suggested that the former subjects directed their attention mainly to regularity and the latter mainly toward ease of movement. According to the record all the subjects were reported to have kept the same speed according to the directions given. Since their attention was fixed more closely on the regularity of the movement any shortening of the interval if it did not materially effect the regularity of the movement would not be noticed.

The interval for each individual, then, varies with the development of automatic control. Unless the subject is directed by some external sound to a certain time interval, until a definite muscular movement has become habitual, the tendency will be to decrease the interval between successive beats until that stage is reached when the movement is guided by the natural rate of the response of the nervous system.

In every instance, at the beginning of the series of experiments, each of the subjects had in mind some musical composition which guided him in beating time. As soon as the subject felt that he could beat time more regularly by being guided by the muscular feeling then the other was given up. When the subject thought of some musical composition the movement was much slower and more irregular than it was when it became automatic.

Preliminary tests with a sounding key showed that when the interval was guided by the ear there was a tendency to emphasize certain beats depending upon their frequency. This, of course, was due to the fact that one is accustomed to emphasize certain sounds in music and speech.

That the physiological rhythm is different from the emphasis rhythm is very clearly established by the results of this set of experiments. When the subject thought of a musical composition the movement corresponded to that which BOLTON[1] defined as the "2-groupings." However, in the latter part of the series, when the movement came to be controlled by the muscular feeling, the rate of movement corresponded to that which he called the "grouping by fours."

Our observations justify us in stating that rhythmical movement rests not only on the rate of breathing, heart-beat, etc., but also on other physiological and mental processes. It is a mental process in that the

---

[1] BOLTON, *Rhythm*, Am. Jour. Psych., 1893 VI 215.

duration between successive beats is mentally estimated; it is a physiological process in that the time interval can be most accurately measured by muscular action.

Relative decrease of error.

In order to see how the error was influenced by practice, the decrease of the error from day to day was computed according to the formula given on page 61. The results are given in Table XIV. The numerals

TABLE XIV.

Daily decrease of error shown in percentages.

Subject.	Section of beats.	Serial number of experiment. 1	2	3	4	5	6	7	Ave
A.F.	I	50	37	5	32	31	0	—9	21
	II	58	33	—12	35	—3	8	15	19
W.S.	I	24	32	20	—12	32	32		21
	II	38	44	49	11	39	17		33
E.F.	I	18	—10	45	29				20
	II	43	34	37	16				32
C.S.	I	34	—38	—34	—33	18	15		—14
	II	46	—47	6	—19	31	30		14
E.W.	I	34	24	26	11				26
	II	17	23	21	20				20
J.P.	I	51	37	23	34				36
	II	44	0	17	20				20
W.J.	I	32	55	33	47	31	17		36
	II	50	44	37	41	17	0		31
	Avg.	38	19	19	17	24	15		

I and II represent the percentages for the first and second groups of 40 beats respectively. The percentages in this table are based on the results given in Table XIII.

I and II, indicate respectively the first and second groups of 40 beats. Briefly, the percentages in that table show the average amounts of gain from the first, second, etc., days until the close of the series of experiments. The averages in the vertical column show that only with one subject, C.S, did practice produce negative results; and this occurred only in the first 40 beats. In the second group of 40 beats, however, the average daily gain in regularity amounted to 14%. Undoubtedly the large variations in the first group were occasioned by the irregularities in the nervous condition of the subject; it was necessary to take him at times when he was very busily engaged at work.

Two general characteristics mark the results given in Table XIV.

First. As rhythmical movement became more automatic, the percentage of decrease in the probable error proportionately decreased. This may be seen, not only by an inspection of the records for each of the

subjects, but is expressed, in a general way, by the average daily percentage of decrease as given at the bottom of the table. The same thing is seen here as in all our previous experiments and that is that the beneficial effect of practice in the reduction of the irregularities of movement is considerably greater in the first experiment than in any succeeding experiments. This decreases with considerable regularity as practice continues from day to day.

Second. The average daily gain in regularity of movement varies with each individual. The averages given in the vertical columns show variations ranging from -14% to $+36\%$. The greater uniformity, as well as the usually larger percentages of the second group of 40 beats show that it requires some time before the muscles can adjust themselves to the steadiest and evenest action. Consequently the period of the first 40 beats may be considered as the training period of the muscles.

Finally, our results show that the principal effects of practice consisted mainly in overcoming the previous effects of emphasis rhythm, the rhythm of speech, poetry and music. As this was overcome the beating-time became more rapid and at the same time more regular.

VII. General summary.

The object of this investigation was to ascertain the results of practice on voluntary movements by repeating the same movements an equal number of times each day until approximately the highest degree of perfection attainable was reached.

I. *Triangular movement of the arm.*

The first experiment consisted in tapping continuously at the corners of an equilateral triangle whose sides measured 20^{cm}. The tests each day lasted only a short time; they were performed from 6 to 11 days by seven persons.

The results of the experiment showed that the greatest gains in rapidity of triangular movements of the hand as well as in the regularity of successive movements were made in the early part of the practice. The percentage of gain in speed rapidly decreased, being 20% for the second day, 10% for the fifth, and 5% for the ninth day. The probable error was used as a measure of irregularity. The percentage of decrease in irregularity of successive movements was not so large in the first part of practice as the percentage of gain in speed; but after the fourth day the percentage of decrease had grown until it exceeded the percentage of increase in rapidity, thus demonstrating that the psychological order of development in voluntary movement is (1) rapidity, and (2) regularity.

The results also showed that during each practice period the subject constantly increased in speed and regularity of movement until the setting in of fatigue. However, where the exercise was continued, after a short interval there was a renewal of the effort and the same results were observed to occur, though the period was much shorter than in the former case. These periods of renewal of energy were observed to become shorter each time until they came to effect almost every alternate movement.

II. *Drawing circles.*

This experiment consisted in making circles with the free arm movement. A true circle, drawn with a compass, 60^{mm} in diameter was placed before the subject as a copy. Preliminary tests showed that ten circles at one sitting gave the best general results. The tests were made on seven subjects, extending over six days.

The results showed that with the right hand most of the subjects gained in smoothness of contour in their drawings both during the progress of each practice and from day to day; with the left hand the results were more irregular.

Though all gained in the smoothness of contour of their curves, yet all did not make them of a size corresponding to that of the copy. These results brought out three types of practice: (1) that in which the subject decreased the size of the circle both during the progress of each experiment and from day to day; (2) that in which the size of the circle was increased during the experiment but decreased from day to day: (3) that in which there was but little variation from the copy either during the progress of the experiment or from day to day. The first two classes were those who regarded more carefully the smoothness of contour of their own drawn curves than they did their correspondence in size to that of the copy. The third class were those who directed their attention more especially to the size of the curve, and who closely observed the copy each time before beginning to draw their own curves.

The results also showed an important principle bearing on pedagogy: that a short exercise often repeated is the best method of practice for rapid development of accurate adjustment of the muscles. Long practice at writing, drawing, etc., seems to be time and energy wasted. Not only are inattentive habits cultivated, but every wrong adjustment gains a place in the chain of subconscious memories, and therefore delays the development of the control over the muscles for accurate adjustments.

III. *Development of control over untrained muscles and less adaptable joints.*

This experiment consisted in tapping continuously with the large toe

until it was completely fatigued. The make and break contacts of an electric key were connected with markers so that each movement of the key was recorded on the smoked surface of a revolving drum. In this way each phase of the toe's movement could be measured; the phases were four, namely, the downward movement, the downward rest, the upward movement and the upward rest.

The average tap-time of the subject studied was on the first day 436^{σ}; this very regularly decreased until at the close of the practice it was 212^{σ}. Likewise, the probable error decreased from 103^{σ} to 35^{σ}. Moreover, the upward rest was longer in the first part of the practice than the other three phases combined; but at the close of the series, it was the same as the downward rest, thus showing that the greatest gains in voluntary activity are those resulting from the practice of the weakest and less exercised muscles.

IV. *Estimation of time.*

After a number of preliminary tests, the intervals, 82^{Σ}, 100^{Σ}, and 164^{Σ} were chosen. The practice lasted from 8 to 16 days on seven subjects.

The results justify the following conclusions: (1) The estimate of a given interval varies for different individuals both with and without practice. (2) Practice on the same interval may cause the variations from the given interval to increase with one person and decrease with another. (3) Time estimate is a personal factor depending upon (a) the nature of the person, whether of an impulsive or quiet temperament, and (b) upon the point of the fixation of the attention, whether to the sensory or the motor side. (4) There is no "indifference point" from which the subject does not vary with long continued practice. The changes that practice produces in the estimation of time are probably due to fixing the attention on the movement to be performed, in which case the estimate is shortened in accordance with the growth of automatic control, or to the sensory side in which case the time-estimate is made longer by practice.

V. *Regulated rhythmical action.*

In arranging apparatus for this experiment the probable error was found for the EDISON phonograph to range from 0.2% to 0.7%; for the LUDWIG kymograph by BALTZAR, from 0.2% to 2%; for a drum run by an EDISON motor driven by carefully tended EDISON-LALANDE batteries, for 0.1% to 3%. The PFEIL marker was found at a break of the circuit to have a latent time ranging from $1.1^{\sigma} \pm 0.09^{\sigma}$ with the magnet cores distant from the armature to $14.7^{\sigma} \pm 0.03^{\sigma}$ with the cores close to

the armature. At a make the latent time ranged from $1.8^\sigma \pm 0.1^\sigma$ to $1.3^\sigma \pm 0.5^\sigma$. With this marker the make is nearly as good as the break except for its slightly greater irregularity. The DEPREZ marker from VERDIN showed a latent time at the break of $3.8^\sigma \pm 0.07^\sigma$ and of $2.5^\sigma \pm 0.64^\sigma$ at the make. Changes in the adjusting spring did not make any great changes in the figures. The probable error of the spark records was found to be $\pm 0.25^{mm}$ independent of the speed of the drum.

In beating time in unison with a sounder click each subject had his own constant error; this was generally negative, that is, the subjects generally beat time before the click occurred. With practice the constant error tended steadily to decrease, to become positive and to increase positively. The irregularity steadily decreased.

VI. *Free rhythmical action.*

The seven subjects were required to beat time without any objective signal. The interval chosen at the start was unintentionally shortened with the progress of the experiment; it was also shortened from day to day. The irregularity decreased in like manner.

NOTES.

The regular courses of the laboratory for the year 1898–99 were as follows:

1. *Physiological and experimental psychology.* Two lectures per week throughout the year. Text-books: LADD'S Outlines of Physiological Psychology, SCRIPTURE'S New Psychology. 65 seniors and juniors, 10 graduates.

2. *Elementary laboratory course.* One exercise a week throughout the year. All students work simultaneously at the same exercise, each step being supervised by the instructor before the next is taken. The course is designed to afford a training similar to that of an elementary course in chemistry or physiology. For the section on sight SANFORD'S Laboratory Course is used as a text-book. 14 seniors and juniors, 7 graduates.

3. *Intermediate laboratory course.* A series of 28 weekly exercises in psychological measurements. The students work in pairs at the exercises in rotation. Each exercise occupies two or three hours. The students learn the methods of measurement and computation and the use of various instruments such as the chronoscope, recording drum, etc. Text-book: SCRIPTURE'S *Elementary course in psychological measurements,* Stud. Yale Psych. Lab., 1896 IV 89–139. 7 seniors and juniors, 3 graduates.

4. *Advanced laboratory course.* Lectures and advanced exercises in the application of elementary mathematics in psychological problems. Text-books: FISHER'S Infinitesmal Calculus, HOLMAN'S Precision of Measurements, WEINSTEIN'S Physikalische Maassbestimungen (part of Vol. I). 4 graduates.

5. *Technical course.* This consists of a series of exercises for those who expect to teach experimental psychology and to manage a laboratory. The instruction covers: the principles involved in making, repairing and caring for apparatus, with practical training in wood and metal work; the methods of experimental demonstration, with practice in the preparation of lantern slides and the use of lime-light and electric lanterns; the principles of laboratory economy, etc. The workshop practice is cared for by a special instructor. The student is expected to make several pieces of apparatus involving the use of the screw-cutting lathe and the various small tools. He is urged to become sufficiently familiar with apparatus and lantern work to successfully give an illustrated lecture; practice lectures are held and subjected to criticism. The director gives special attention to fitting the men in this course for college positions. 5 graduates.

6. *Research-work in psychology.* Participants in this course are either investigators or assistants. For assistants the object is such a training in accurate introspection, observation, experimenting and the art of research as is desirable for the general psychologist. This work is open to all. Only those who have had sufficient experience are permitted to undertake independent investigations. The result of all investigations belong to the archives of the laboratory. Those who undertake investigations thereby agree to prepare the results for publication, subject to approval, in the *Studies from the Yale Psychological Laboratory.* 6 graduates (independent investigators).

7. *Applied psychology.* One hour per week throughout the year. Application of modern psychological principles to educational subjects; outlines of the psychology of touch, its use in education; motor abilities, accuracy of movement, fundamental principles of writing and drawing; sight, color-teaching; space, form-teaching, drawing,

modeling; attention, concentration and distraction, laws for developing attention; memory, analysis into its components, experimental study of, develment and training, systems of mnemonics, time of study; imagination, use, necessity of development and repression; emotions, will; action, reflex, automatic, instinctive, voluntary, their training; education of the blind, the deaf and other defectives; principles of anthropometry and psychometry applied to study of scholars; psychological development, beginnings of instruction; economy in education, greatest results from least efforts, correlation and concentration of instruction. The course is illustrated with experiments, lantern views, and a large collection of educational material from Europe and America. 24 seniors and juniors, 9 graduates.

Some experiments by WEDENSKY, *Contribution à l' étude de l' innervation centrale*, III. internationaler Congress für Psychology, appear to have a bearing on the explanation of the work on cross-education that is being carried on at Yale. Professor WEDENSKY experimented on the cortical motor centers for the anterior limbs of the dog and the cat with the result that the state of excitation of one center played an important rôle in the modification produced by stimulation of the symmetrical center.

The date of FECHNER's paper should be given on page 6 as 1858 instead of 1758.

STUDIES

FROM THE

Yale Psychological Laboratory

EDITED BY

EDWARD W. SCRIPTURE, Ph.D.
Director of the Psychological Laboratory

1899

VOL. VII.

YALE UNIVERSITY
NEW HAVEN, CONN.

PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

CONTENTS.

Researches in experimental phonetics, by E. W. SCRIPTURE, . 1

Observations on rhythmic action, by E. W. SCRIPTURE, . . 102

RESEARCHES IN EXPERIMENTAL PHONETICS

(*First Series*)

BY

E. W. SCRIPTURE.

The science of speech is at the present moment passing into the phase of experiment. For many years experiments have been made on the vowel sounds and on similar topics from a physical point of view, but it is only recently that the attempt has been made to arrange systematic work exclusively for the purposes of a science of speech itself.

The present study, begun in October, 1897, gives the account of some of the results already obtained (to the end of 1899) in the system of researches now in progress in the Psychological Laboratory of Yale University. The scope of these researches is far wider than the topics considered in this first report. "Experimental phonetics" would include the material of the present study but such a term would need to be extended beyond its present significance to include all the work now in progress here. I believe, however, that there will be no objection to using the name "experimental phonetics" for a science of speech in all its forms as a matter of expression. This would include not only speech sounds as material for language, but also their changes resulting from different mental conditions such as fatigue, emotion and the like; it would also include the study of rhythm in speech with its application in poetry and music.

The present investigation owes its immediate origin to suggestions from and discussions with Prof. T. D. GOODELL (Greek) and Prof. HANNS OERTEL (Comparative Philology). The question was raised concerning the possibility of using laboratory methods to settle the controversy in regard to the quantitative character of English verse. It was finally decided to study some records of English poetry made for one of the talking machines. After various trials it was found possible to obtain speech records in such a way that they could be measured.

It quickly became apparent that work on this problem required preliminary work on the elementary sounds of language. This work led to so many novelties and showed so clearly the need of revising many of our concepts of the nature of speech that the original problem was postponed

until the most valuable facts in regard to spoken sounds could be collected. These facts lay before me immediately in the records; it was only necessary to measure the sound curves and interpret them. This measuring was a most laborious and fatiguing process but after a month or two of practice in interpreting the curves the work proved to be incredibly profitable; it was rare to spend an hour at work on them without discovering some new fact. The field is, indeed, so rich and so unexplored that there is unlimited gain for any one wishing to enter it. To any one wishing to use the same methods every possible facility will be afforded by the Yale laboratory.

I. Apparatus for studying speech records.

The choice of a method for obtaining measurable records seemed to lie between:

1. Causing the sound to trace a record that might be directly studied, without the possibility of reproducing the sound.

2. Causing the sound to trace a record which could be used to reproduce the sound and which could also be studied.

Both of these principles involved most serious difficulties; a long series of investigators and inventors had, however, rendered them possible.

The *former principle* appears to have been first applied by Scott in his phonautograph.[1]

In Scott's phonautograph a large parabolic receiving trumpet carries at its end a thin membrane whose movements cause a small recording lever to write upon the smoked surface of a cylindrical drum. The sounds of the voice passing down the receiver agitate the membrane and cause the lever to draw the speech curve on the drum. A vibrating fork serves to write the time line beside the speech line. Scott was a typographer and afterwards a dealer in photographs; the instrument was made by Rudolph Koenig, the well-known maker of acoustical apparatus in Paris.

The instrument as improved by Koenig was used by Donders and others.[2]

The logograph of Barlow consisted of a trumpet or mouthpiece end-

[1] Scott, Inscription automatique des sons de l'air au moyen d'une oreille artificielle, 1861.

Scott, *Phonautographe*, Annales du Conservatoire des Arts et Metiers, Oct., 1864.

Scott, *Phonautographe et fixation graphique de la voix*, Cosmos, 1839 XIV 314.

Lippich, *Studien über d. Phonautographen von Scott*, Sitzb. d. Wien. Akad., Math.-naturw. Kl., 1864 L (II. Abth.) 397.

[2] Donders, *Ueber d. Natur aer Vokale*, Arch. f. d. holländ. Beiträge z. Natur.- u. Heilk., 1858 I 157.

ing in a thin membrane of rubber. A thin lever of aluminum carrying a point dipped in color wrote the speech curves on a band of paper.[1]

A still further improved phonautograph was used by SCHNEEBELI,[2] which carried two points, one fixed to aid in comparison and the other moving with the membrane. The inscription was made on a light strip of glass covered with a light coating of smoke and drawn on a carriage rapidly in front of the recording points. The tracings were measured with the aid of micrometric screws. SCHNEEBELI gives a number of the characteristic curves of the vowels.

Various similar methods have been employed with constantly better results. The ear drum has been used for the membrane by C. BLAKE.[3]

The hindrance due to the inertia of material levers was avoided by E. W. BLAKE, who attached a mirror to a telephone plate in such a way that a beam of light was deflected by each movement. A ray of light from a heliostat was reflected through lenses upon a photographic plate moving with a constant velocity. The sound wave thus recorded a line on the plate.[4]

PREECE AND STROH used a thin membrane of rubber stretched by a cone of paper. The cone was made to move a fine glass tube supplied with an aniline ink, the record being taken on a band of paper.[5]

RIGOLLOT ET CHAVANON covered the wider end of a paraboloid with a very thin membrane of collodion, to the center of which was fixed a small mirror working on an axis of fine thread. The deflections of the ray of light were recorded on a sensitive paper.[6]

DONDERS, *Zur Klangfarbe der Vokale*, Arch. f. d. holländ. Beiträge z. Natur. u. Heilk., 1861 III 446.

DONDERS, *Zur Klangfarbe der Vokale*, Ann. d. Phys. u. Chem., 1864 CXXIII 527.

DONDERS, De physiologie der spraakklanken, Utrecht 1870.

SCHWAN UND PRINGSHEIM, *Der französische Accent*, Arch. f. d. Studium d. neueren Sprachen, 1890 LXXXV 203.

[1] BARLOW, *On the pneumatic action which accompanies the articulation of sounds by the human voice, as exhibited by a recording instrument*, Proc. Roy. Soc. London, 1874 XXII 277.

BARLOW, *On the articulation of the human voice, as illustrated by the logograph*, Proc. Roy. Dublin Soc., 1880 N. S. II 153.

[2] SCHNEEBELI, *Expériences avec le phonautographe*, Arch. des Sciences phys. et nat. de Genève, 1878 (Nouvelle période) LXIV.

SCHNEEBELI, *Sur la theorie du timbre et particulièrement des voyelles*, Arch. des Sciences phys. et nat. de Genève, 1879 (III. période) I 149.

[3] BLAKE, *The use of the membrana tympani as a phonautograph and logograph*, Archives of Ophthal. and Otol., 1876 V No. 1.

[4] BLAKE, *A method of recording articulate vibrations by means of photography*, Amer. Jour. Sci., 1878 XVI 55; also in Nature, 1878 XVIII 338.

[5] PREECE AND STROH, *Studies in acoustics*, Proc. Roy. Soc. London, 1879 XXVIII 358.

[6] RIGOLLOT ET CHAVANON, Journal de physique, 1883 553.

The most highly developed instrument of the lever recording type seems to be that of HENSEN.[1] It consists of a membrane of goldbeater's skin in a conical form produced by molding it over a shape while moist and allowing it to dry before removal. A single light lever attached to the center of the membrane carries a fine glass thread as a recording point. It writes the curve on a thinly smoked strip of glass. The curves are studied with a microscope. This instrument has been used in several investigations.[2]

An important improvement was made in HENSEN'S recorder by PIPPING who replaced the glass thread by a small diamond which scratched the curve directly on the glass strip. With this instrument PIPPING has made a series of investigations, chiefly on the vowels.[3]

RAPPS also avoids the difficulties of a diaphragm or membrane by an ingenious optical method.[4]

The MAREY tambours in various modifications have been frequently used.[5] Other devices have been employed at different times.[6]

---

[1] HENSEN, *Ueber die Schrift von Schallbewegungen*, Zt. Biol., 1887 XXIII 291; first described by GRUETZNER, Physiologie d. Stimme u. Sprache, 187, in HERMANN'S Handb. d. Physiol., I. Bd., II. Theil, Leipzig 1879.

[2] WENDELER, *Ein Versuch, d. Schallbewegung einiger Consonanten u. anderer Geräusche mit d. Hensen'schen Sprachzeichner graphisch darzustellen*, Diss. Kiel, 1886; also in Zt. f. Biol., 1887 XXIII 303.

MARTENS, *Ueber das Verhalten von Vokalen und Diphthongen in gesprochenen Worten*, Diss. Kiel, 1888; also in Zt. f. Biol., 1889 XXV 289.

[3] PIPPING, *Om Klangfärgen hos sjunga vokaler*, Diss. Helsingfors, 1890; also as *Zur Klangfarbe d. gesungenen Vokale; Untersuchung mit Hensens Sprachzeichner* (Diss. in Swedish, Helsingfors 1890), Zt. f. Biol., 1890 XXVII 1.

PIPPING, *Nachtrag zur Klangfarbe der gesungenen Vokale*, Zt. f. Biol., 1890 XXVII 433.

PIPPING, *Zur Lehre v. d. Vocalklängen*, Zt. f. Biol., 1895 XXXI 525.

PIPPING, *Phonautographische Studien über d. Quantität schwedischer Worte u. d. musikalischen Accent*, Finländska Bidrag. till Svensk Språk och Folklifsforskning, Helsingfors 1894.

PIPPING, *Ueber d. Theorie d. Vokale*, Acta Societatis Scientiarum Fennicæ, 1894 XX No. II.

[4] RAPPS, *Ueber Luftschwingungen*, Diss., Berlin 1892; also in Ann. d. Phys. u. Chem., 1893 L 193.

[5] ROUSSELOT, Les modifications phonétiques du langage, Paris 1892.

BOURDON, *L'Application de la méthode graphique à l'étude de l'intensité de la voix*, Année psychol., 1897 IV 369.

WAGNER, *Französische Quantität (unter Vorführung des Albrecht'schen Apparats)*, Phonet. Studien, 1893 VI 1.

[6] FICK, *Zur Phonographik*, Beiträge zur Physiologie LUDWIG gewidment, 23, Leipzig 1887.

KOSCHLAKOFF, *Die künstliche Reproduction u. graphische Darstellung d. Stimme*, Arch. f. d. ges. Physiol. (Pflüger), 1881 XXXIV 38.

The manometric flame method was devised by KOENIG.[1] The vowel is sung or spoken into the trumpet leading to the small box known as the manometric capsule. This box is divided in two parts by a thin rubber membrane. The part opposite the trumpet is a tight chamber through which illuminating gas is flowing; the gas is lighted at the end of the small tube. As the sound waves descend they strike the rubber membrane, set it in vibration and thus produce movements of the gas analogous to those of the air in the sound waves. By means of a revolving mirror the vibrations of the flame can be seen. These flames can be photographed[2] by selecting the right composition of the illuminating gas; cyanogen gas has been used; a mixture of hydrogen and acetylene gas burning in a chamber of oxygen seems to be successful.

The foregoing methods have been employed for the solution of the most diverse problems.[3]

The *second principle* is that of the sound-reproducing machines, or talking machines.

The original talking machine seems to have been the phonograph of EDISON. The tin-foil phonograph was afterwards superseded by the wax-cylinder form.

A sheet of thin glass receives the sound waves and engraves them in a surface of hard wax by means of a sapphire knife attached to it. By replacing the sapphire knife with a round sapphire point the glass diaphragm is made to reproduce the sound.

The great advantage of this method lies in the fact that the record can be made audible at any time; the accuracy of the result can thus be always tested.

---

[1] KOENIG, *Die manometrischen Flammen*, Ann. d. Phys. u. Chem., 1872 CXLVI 161.

KOENIG, Quelques expériences d'acoustique, 46, Paris, 1882.

AUERBACH, *Untersuchungen ü. d. Natur. des Vokalklangs*, Diss. Berlin, 1876; also in Ann. d. Phys. u. Chem., 1876 Ergänzungsbd. VIII.

[2] STEIN, in MAREY, La methode graphique, p. 647.

DOUMER, *Mesure de la hauteur des sons par les flammes manométriques*, C. r. Acad. Sci. Paris, 1886 CIV 340.

DOUMER, *Études du timbre des sons, par la méthode des flammes manométriques*, C. r. Acad. Sci. Paris, 1887 CV 222.

DOUMER, *Des voyelles dont le caractère est très aigu*, C. r. Acad. Sci. Paris, 1887 CV 1247.

MARAGE, *Études des voyelles par la photographie des flammes manometriques*, Bull. de l'Acad. de Med., 1897 XXXVIII 476.

NICHOLLS AND MERRITT, *Photography of manometric flames*, Physical Review, 1898 VII 93.

[3] AUERBACH, *Die physikalischen Grundlagen der Phonetik*, Zt. f. franz. Sprache u. Lit., 1894 XVI 117.

ROUSSELOT, Principes de Phonétique Expérimentale, Paris 1897.

The phonograph has been used to receive records which have afterwards been studied.

The methods of studying phonograph records are of two kinds. Direct enlargement and measurement by means of the microscope is the method followed by BOEKE.[1] Enlargement by means of amplifying levers, recording directly on a smoked cylinder is the method used by a series of observers.[2] Phonograph records have been studied to a considerable extent.[3]

Enlargement by means of levers recording on photographic paper by means of a beam of light is the method developed by HERMANN.[4] The Yale laboratory is equipped for this method also.

---

[1] BOEKE, *Mededeeling omtrent onderzoekingen van klinkerindruskels op de wasrollen van Edison's verbeterden fonograaf*, De natuur, 1890, July.

BOEKE, *Mikroskopische Phonogrammstudien*, Arch. f. d. ges. Physiol. (Pflüger), 1891 L 297.

MEYER, *Zur Tonbewegung des Vokals im gesproch. u. im gesung. Einzelwort*, Phonet. Studien, 1897 X 1 (Neuere Sprachen, IV).

[2] MAYER, *Edison's talking machine*, Nature, 1878 XVII 469.

FICK, *Zur Phonographik*, Beiträge zur Physiologie LUDWIG gewidmet, 23, Leipzig 1887.

JENKIN AND EWING, *The phonograph and vowel theories*, Nature, 1878 XVIII 167, 340, 394.

JENKIN AND EWING, *On the harmonic analysis of certain vowel sounds*, Trans. Roy. Soc. Edinb., 1878 XXVIII 745.

KLUENDER, *Ueber d. Genauigkeit der Stimme*, Arch. f. d. ges. Physiol. (Pflüger), 1879 I 119.

LAHR, *Die Grassmann'sche Vokaltheorie im Lichte des Experiments*, Diss., Jena 1885; also in Ann. d. Phys. u. Chem., 1886 XXVII 94.

M'KENDRICK, *On the tone and curves of the phonograph*, Jour. Anat. and Physiol., 1896 XXIX 583.

M'KENDRICK, MURRAY AND WINGATE, *Committee report on the physiol. application of the phonograph and on the form of the voice curves by the instrument*, Rept. Brit. Ass. Adv. Sci., 1896 669.

WAGNER, *Ueber d. Verwendung d. Gruetzner-Marey'schen Apparats u. d. Phonographen zur phonetischen Untersuchungen*, Phonet. Studien, 1890 IV 68.

[3] MARICHELLE, La parole d'après le tracé du phonographe, Paris 1897.

GELLE, L'audition, Paris 1897.

MARAGE, *Les phonographes et l'étude des voyelles*, Anneé psychol., 1898 V 226.

[4] HERMANN, *Phonophotographische Untersuchungen*, I., Arch. f. d. ges. Physiol. (Pflüger), 1889 XLV 582.

HERMANN, *Ueber d. Verhalten d. Vokale am neuen Edison'schen Phonographen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 42.

HERMANN, *Phonophotographische Untersuchungen*, II., Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 44.

HERMANN, *Phonophotographische Untersuchungen*, III., Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 347.

Another of the talking machines is the gramophone. This is a development of the recording idea contained in SCOTT's phonautograph in combination with the idea of reproducing the sound in a special manner. The inventor of the method is Mr. EMIL BERLINER, of Washington, D. C. The United States patents covering the apparatus and processes are as follows: Gramophone, No. 372,786, Nov. 8, 1887; Process of producing records of sound, No. 382,790, May 15, 1888; Gramophone, No. 534,543, Feb. 19, 1895; Sound-record and method of making same, No. 548,623, Oct. 29, 1895; Gramophone, No. 564,586, July 28, 1896. These patents can be readily found in the annual reports published by the United States Patent Office.

The researches to be now reported have been made with the aid of the gramophone; an acquaintance with the principles involved in the production of the gramophone records is necessary to the proper understanding of the results obtained.

1. Making gramophone plates.

For convenience the apparatus may be divided into two sections, the recorder and the impression disc.

The recorder with which I am acquainted is that described in the Letters Patent No. 564,586; it is shown in Fig. 1. The recorder comprises a thin glass diaphragm held in a frame, Fig. 2. This frame opens on one side into a speaking tube. It is cut away on the other side to afford connection with the recording stylus. From the center of the diaphragm a metal post rises, whose free end has an axial slot into which a piece of soft rubber tube is forced and flattened. The free end of the tube receives the metal stylus, which extends outward radially and ends in a flat, sharp, flexible point. Near the middle of the stylus a hole is bored and a pin formed at one end of a metal block passes through the hole and into the central bore of a similar block. Between each block and the stylus there is a soft rubber washer. The blocks are made to clamp the stylus by means of the pointed screws passing through the support and serving as pivots.

HERMANN, *Bemerkungen zur Vokalfrage*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVIII 181, 543.

HERMANN, *Phonophotographische Untersuchungen*, IV., *Untersuchungen mittels des neuen Edison'schen Phonographen*, Arch. f. d. ges. Physiol. (Pflüger), 1893 LIII 1.

HERMANN UND MATTHIAS, *Phonophotographische Mittheilungen*, V., *Die Curven d. Consonanten*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 255.

HERMANN, *Phonophotographische Untersuchungen*, VI., *Nachtrag zur Untersuchung der Vocalcurven*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 264.

HERMANN, *Weitere Untersuchungen ü. d. Wesen d. Vocale*, Arch. f. d. ges. Physiol. (Pflüger), 1895 LXI 169.

These pivots form the fulcrum of the stylus. The stylus is dampened by a piece of soft rubber inserted between it and the metal cover of the sound box.

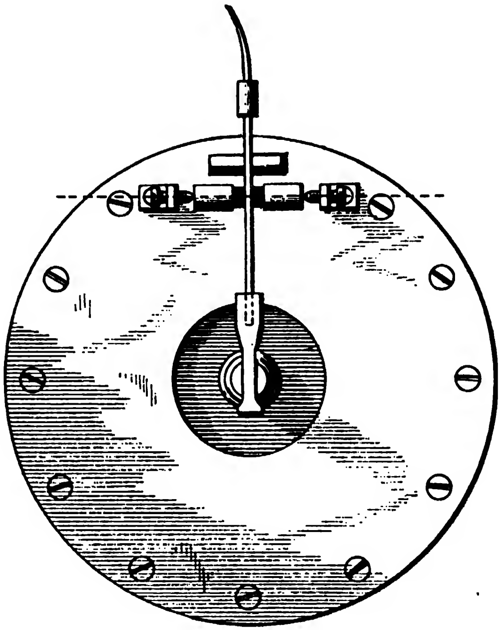


Fig. 1.

The sound waves coming down the speaking tube set the diaphragm in motion; this diaphragm moves one arm of the stylus and the point at the end of the other arm repeats this movement.

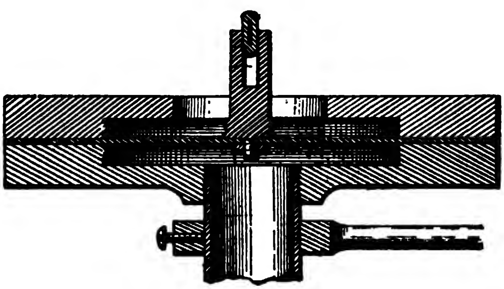


Fig. 2.

The impression disc is prepared by two methods. I shall describe first the method with which I am acquainted and then a later method which seems of special interest.

In the former method (Patent No. 382,790) a highly burnished zinc disc 18^{cm} in diameter is flowed with a saturated solution of wax in benzine; the film of wax thus deposited is so thin that the touch of a camel's hair brush marks it perceptibly.

The prepared disc is placed on a revolving plate so that its surface is touched by the point of the recording stylus (Patent No. 534,543). As the plate revolves the recorder is made to travel toward the center; thus its point cuts a spiral groove through the wax. The vibrations of the point make deflections in this groove. These deflections are in the plane of the surface of the plate and not dug into it as in the case of the phonograph.

The record disc is then placed in an etching bath similar to that used by photo-engravers (Patent No. 548,623). The part of the zinc from which the wax has been removed by the stylus is attacked by the acid and a permanent groove is made. A copper matrix is then made from this by electrolysis. The matrix contains the sound-line in relief. After the matrix has been protected by a layer of nickel, unvulcanized rubber is pressed into it. The rubber is then vulcanized in place. When removed from the matrix the rubber plate is a true copy of the original disc.

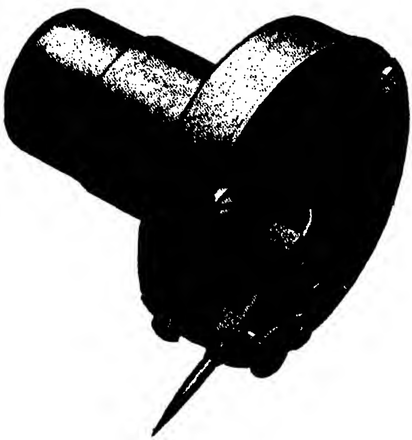


FIG. 3.

The later method of making record discs I know only from a study of the Letters Patent, No. 564,586. I judge, however, that it is a better method and I believe that it may be of easy application in the direct study of records by the microscope.

In this method a glass plate is clamped on an axis by which it can be rotated. The under-surface of the disc is carefully polished and dried

and is then covered with a thin film of linseed oil by means of a camel's hair brush. A smoky flame then held under the plate deposits a fine layer of lamp-black, thus forming an amorphous ink which covers the glass in an even, exceedingly thin layer. This coating of ink does not flow spontaneously and requires only a minute force to trace a line in it. The sound line is drawn by the point of the recording stylus in a manner similar to that just described. Copies of the disc are made by placing it over a sensitized photographic plate and proceeding by photo-engraving.

To reproduce the sound the rubber disc is placed on a plate which can be rotated by some motor power. A reproducing sound box is so arranged that the point of its stylus travels in the sound-groove. The deviations in the sound groove move the point of the stylus whereby a glass diaphragm is made to reproduce the sound waves. The reproducing sound box differs from the recording sound box chiefly in having a stiff round steel point at the end of the stylus instead of a cutting point, as shown in Fig. 3.

2. *Transcribing gramophone records.*

The speed at which the plate travels in the record-making machine is about 70 revolutions a minute. This stretches out the curves for the speech sounds so that the variations in amplitude are visible through the microscope only in the case of musical sounds and vowels. The method of direct reading by the microscope is therefore not available. The record must be transcribed in such a way that the relation between length and height, that is between time and amplitude, shall be changed. In the method about to be used the height was enlarged while the length was decreased.

In the transcribing apparatus (Fig. 4) the gramophone plate was put on a metal disc *E* similar to that of the original record-making machine. This disc was rotated at a speed of 0.1 revolution a minute by a system of spur and bevel gears. The particular system used was adopted after long experimenting; as it may be of use to others it may be profitable to briefly describe it.

A small 110-volt Edison motor *A* was connected with the electric mains through an appropriate resistance. A convenient and cheap form of resistance *L* was found in the so-called reduction sockets for 16 c. p. lamps. These contain fine resistance wire wound on asbestos, which can be placed in circuit with the lamp to any desired extent, thereby reducing the current passing through it. An appropriate plug carrying the motor wires was placed in one of these sockets; this socket was connected to another plug which was placed in another reduction socket; this finally was con-

nected to a plug placed in a socket on the main line. By moving the knobs on the reduction sockets the speed of the motor could be reduced as desired. Finally the current was passed through a 4 c. p. lamp as a permanent resistance of 800 ohms. In making the present records the motor was adjusted to about 800 revolutions a minute.

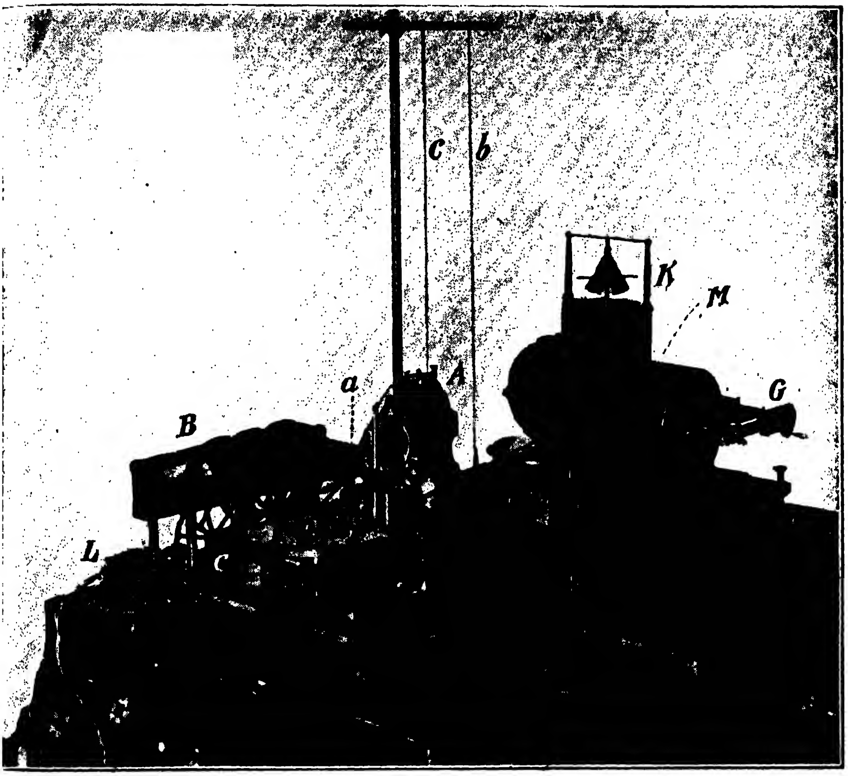


Fig. 4.

A miter gear *a* on the axle of the motor fitted into another miter gear on the first axle of the reducing machine *B*. The first axle of the reducing machine thus revolved at 800 revolutions per minute. (For still finer work it has been found convenient to use a worm on the motor axle and a worm gear on the first reducing axle; for a worm gear of n teeth the speed of the first axle is $1/n$ that of the motor.) The second axle carried a large spur gear with 160 teeth which fitted into small spur gear with 16 teeth on the first axle; thus the second axle made 80 revolutions per minute. In a similar way gear-transmission to a third axle reduced the.

speed to 8 revolutions, and transmission to a fourth axle reduced it to 0.8 of a revolution. This fourth axle carried a spur gear of 20 teeth which fitted into the 160 teeth of the final driving machine of the disc whose axle thus made 0.1 revolution a minute.

The axle of the final driving mechanism carried on its further end a tube *C* with a longitudinal slit in it. Within this tube was a rod 1^{cm} in diameter with a thread of 96 turns to the inch on its surface; it was held by a nut correspondingly threaded. A projection from the rod fitted into the slit in the tube; thus the rod was forced to turn with the tube. At the same time the thread on its surface forced it to move lengthwise $\frac{1}{96}$ of an inch for each revolution. The rod bore on its end a carefully centered point and just back of this point a miter gear. The point pressed against the disc-carriage. This carriage consisted of a bar of brass running on a pair of rails and carrying the metal wheel *E*. The metal wheel rested on the carriage and its axle projected through it. As the rod traveled forward it pushed the carriage ahead of it. At the bottom of the axle there was a second miter gear *D* bearing against the first one on the rod; this turned the metal wheel in unison with the rod. When a gramophone plate was clamped on the wheel with proper centering, it was turned once in 10 minutes and was driven forward radially $\frac{1}{96}$ of an inch per revolution. Thus the speech curve on a plate would travel steadily under a fixed point from beginning to end.

Just above the disc the amplifying lever *F* was adjusted so that the soft steel point rested in the sound groove. The distance from the fulcrum to the point was 22^{mm}. The lever possessed side movement in order to transcribe the curve, and vertical movement in order to follow the changes in the thickness of the plate. The long arm of the lever reached 595^{mm} beyond the fulcrum. The extreme part of it consisted of a recording point of pendulum ribbon *M* 152^{mm} long. This point traced the side movement on the smoked paper and also yielded to the up and down fluctuations without any noticeable effect on the records. The amplification was approximately 27 times.

It was afterwards found desirable to replace the simple supporting adjustments of the steel point by an adjusting standard such as is used in ordinary laboratory work. The point could be raised or lowered by a rack and pinion and adjusted sidewise by a small screw. The vertical movement was convenient for regulating the pressure of the recording points on the drum; the rubber gramophone plates varied in thickness and would consequently raise the point more at one side than the other. This variation has been avoided in the most recently made plates.

The centering of the gramophone plate was not an easy matter. The

speech curve was made in the form of a spiral around the center of rotation in the original machine; neither the edge of the rubber disc with the record nor the hole in its center coincided with this center. To center the spiral accurately on the metal plate two methods could be used. The microscope method proved somewhat the more convenient. The metal disc was moved away from the point of the rod. A microscope or a large magnifying glass was fixed so that it was focussed on the spiral groove. As the disc was turned the groove passed through the field of vision. If the plate was not centered, it would move to one side or the other during one half a revolution; it was adjusted by the fingers until the groove did not appear to move back and forth with every turn, but to maintain a steady side movement amounting to once the width between lines for one revolution. The other method consisted in turning the disc with the recording point adjusted and noting the deviation to one side for one half a revolution. The disc was then moved radially until the point marked one half the deviation. If this was properly done, the point would show no deviation as the disc is turned.

The steel point was pressed into the groove of the plate by means of the rubber band on the thread *b*; the verticality of the pressure was assured by the plumb line *C*.

The record was made on smoked paper moved by the BALTZAR kymograph *K* in the usual way with side movement of the drum by the driving mechanism *G*.

There were such minor adjustments of recording points, levers, etc., as were requisite for accuracy and convenience. To avoid jarring through the floor the table was at a later date suspended from the ceiling by wires. The jarring of the motor was avoided by placing it on sand. The slight variations in the potential of the city current did not appreciably affect the record.

In the laborious work of transcribing these records I was greatly aided by Mr. Minosuke Yamaguchi.

The records were measured with a scale graduated in 10ths of a millimeter under a watchmaker's eye-glass or under a magnifying glass. Thus 0.1^{mm} was the unit of measurement. This represented an interval of time of $0.003\frac{1}{2}^{s}$, or $0.3\frac{1}{2}^{\sigma}$. In the case of regularly repeated vibrations the determination could be made still finer by measuring a long series of vibrations. In the calculations only the tenth of a millimeter was used. The tenths of a sigma in the results may be out by one or two units; thus a series of vibrations recorded as 2.1^{σ}, 2.1^{σ}, 1.9^{σ}, 1.9^{σ}, etc., would be possibly more correctly given as 2.1^{σ}, 2.0^{σ}, 1.9^{σ}, 1.9^{σ}, etc. These steps disappear in the plotted curves of results which were drawn smoothly by aid of rubber curves.

The calculation was aided by ZIMMERMANN'S Rechentafeln and a table of reciprocals. Thus millimeter measurements were turned into periods of vibration by using the table for 35, and frequencies were found by taking the reciprocal of the period.

The reproductions of speech curves in this study were obtained by having the originals photographed, with an enlargement of four times, directly on a wooden block; the engraver then cut the line with his tool. As some of the finer details were necessarily lost in this way, the attempt was made to get larger amplification in the records. Six months of unsuccessful work with compound levers were followed by an attempt (Dec., 1899) with a single very long lever of straw having the fulcrum close to one end and the recording point of glass. This method gives most beautiful curves of the greatest delicacy; they are as large as the curves shown in the figures for *ai*, etc. below and can be reproduced directly by zinc etching. This method is being used for further researches. Many other improvements have also been lately introduced.

In addition to the illustrations produced by photography and cutting by the engraver, others have been made by drawing with the free hand on a very large scale the curve as seen through the magnifying glass; in this way the finer details could be brought out with great accuracy.

II. THE DIPHTHONG *ai* FOUND IN THE WORDS *I, eye, die, fly, thy.*

The words first studied in the present case are those of WILLIAM F. HOOLY, a trained speaker, reciting the nursery-rhyme entitled "The Sad Story of the Death and the Burial of Poor Cock Robin." The record is contained on the plate numbered 6015 made by the National Gramophone Company of New York. As it is impossible to get any definite idea of how the words actually sound except by putting the plate in the gramophone, I will try to indicate some of the characteristics of the words heard.

MR. HOOLY speaks in what appears to be the normal American accent in the neighborhood of New York except in two respects: 1. he makes an unusual effort at distinctness; 2. he recites in the manner frequently adopted by adults in speaking to children—a manner that I am able to characterize only as having an excess of expressiveness and melodiousness.

The record on the gramophone plate, as far as it has been traced off, reads as follows:

Now, children, draw your little chairs nearer so that you can see the pretty pictures and Uncle Will will read to you the sad story of the death and the burial of poor Cock Robin.

Who killed Cock Robin?
    I, said the sparrow,
    With my bow and arrow.
I killed Cock Robin.

Who saw him die?
    I, said the fly,
    With my little eye
I saw him die.

Who caught his blood?
    I, said the fish,
    With my little dish
I caught his blood.

Who'll make his shroud?
    I, said the beetle,
    With my thread and needle
I'll make his shroud.

Who'll be the parson?
    I, said the rook,
    With my little book
I'll be the parson.

Who'll dig his grave?
    I, said the owl,
    With my spade and trowel
I'll dig his grave.

Who'll carry the link?
    I, said the linnet,
    I'll fetch it in a minute.
I'll carry the link.

To extend the treatment to prose some cases of *I* were studied in another record by Mr. WILLIAM F. HOOLEY, entitled "Gladstone's Advice on Self-Help and Thrift," being record number 6014 of the gramophone series. The speech runs as follows:

"Ladies and gentlemen, the purpose of the meeting on the 14th instant may, I can say, be summed up in a very few words: self-help and thrift."

Two examples of this diphthong were also studied in the word *thy*, as it appears in record number 668 Z (name of speaker not given), which runs as follows:

Our Father, which art in Heaven; hallowed be Thy name, Thy kingdom come . . .

In order to get some idea of the relation between the character of the vibrations and the mental character of the word I have recorded judgments

as to how the words appear to the ear. The statements are given with appended initials in the accounts of the various words; the persons observing were: (O), Hanns Oertel; (E. M. C.), Miss E. M. Comstock; (E. W. S.), E. W. Scripture.

ai in the word *I* (first example).

The first occurrence of *ai* is in the verse *I, said the sparrow.*

A reproduction of the curve for this word is given in Fig. 5: As explained on p. 14, some of the details are lost in making the figure and others are not quite correctly given; the original curve is much sharper and clearer.

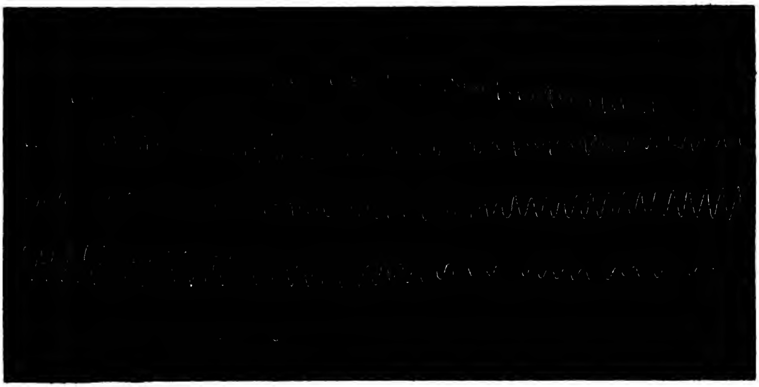


FIG. 5.

This word *I* occupies an interval of 452^{σ} ($^{\sigma} = 0.001^{s}$). It is preceded by a silent interval of 770^{σ}, or about ¾ of a second; this is the full stop in the stanza after the question is asked and before the answer is given, indicated by ? in print. It is followed by a silent interval of 210^{σ}, indicated in print by a comma.

Beginning.—The beginning of the *a* is apparently clear, that is, it is not preceded by any breathing. The vocal cords are apparently adjusted for voice production before the expiration begins; the vowel starts with a light vibration of the cords. There is no explosive sound, or glottal catch, before the vowel.

Pitch.—Beginning with a period of 18^{σ}, the cord tone changes slowly through 11, 10, 9, 8, 7^{σ}, reaching 6^{σ} at the 11th vibration, 5^{σ} at the 15th, 4^{σ} at the 30th; the period of 4^{σ} is maintained to about the 100th vibration, after which it falls slightly to 4.2^{σ} during the last 7 vibrations. In other words, the pitch glides slowly upward from a tone of 56 complete vibrations per second to one of 200 per second, then more slowly to one

of 250 per second, at which pitch it remains constant except for a slight drop as the diphthong ends. Fig. 6 shows the course of the pitch-changes

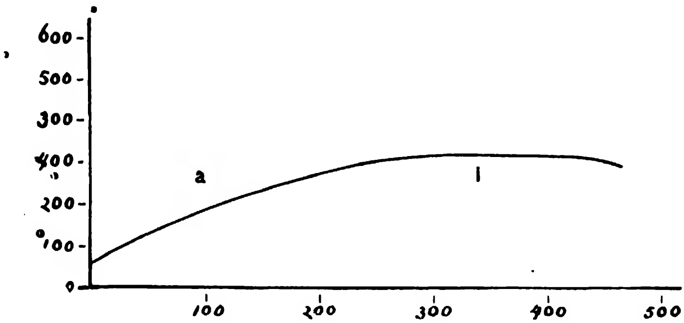


FIG. 6.

during this word. The horizontal axis in this figure, as well as in all similar ones, represents time. The point $x = 0$ is taken at the moment of the first vibration and the sound curve is supposed to be laid along the X-axis. At each point on this axis at which the curve shows a cord vibration to begin an ordinate is erected, inversely proportional to the time from this moment to the beginning of the previous vibration, that is, to the frequency of the cord vibration at that point. By an oversight the figures 300 and 400 have been interchanged.

Formation.—A drawing of the first three vibrations is given in Fig. 7; the dots indicate intervals of 1^{σ}.

FIG. 7.

The vowel *a* begins with a movement of the vocal cords by which an extremely weak puff of air is emitted. This puff of air passing through the resonance-chamber of the mouth arouses 3 or 4 vibratory oscillations of air contained in the chamber. There is first a half oscillation of weak amplitude, then a comparatively strong oscillation, followed by very weak ones. Even the strongest is, however, very weak; the following oscillations are so weak as to be hardly perceptible. The resonance vibrations disappear and there is an interval of silence before the second puff appears. Then the cords emit another puff of air a trifle stronger than the first, the time from puff to puff being 18^{σ}. The 6 resonance vibrations are slightly stronger than before. The period of silence is shorter than before. The third puff occurs 11^{σ} after the second one. The

resonance vibrations are a trifle stronger still; there are 7 of them with a brief interval of silence. The fourth puff begins at 10^{σ} after the beginning of the third one. The fourth puff contains 8 resonance vibrations, all slightly stronger than before; there is no interval of silence because the fifth puff begins just as the last resonance vibration of the fourth puff ends. The interval occupied by the fourth puff is 9^{σ}. The end of the fourth puff, the whole of the fifth puff and the beginning of the sixth are shown greatly enlarged in the drawing, Fig. 8.

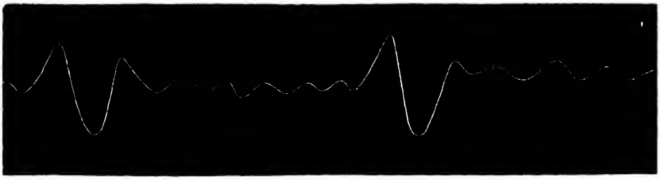


FIG. 8.

It is a characteristic trait of this particular *a* that the vibration is strongest at the start; this indicates a sudden and complete opening of the cords. The quickest opening requires, however, a little time and there must be a measureable change from no passage of air to full passage; this is shown by the weak half of the first resonance vibration preceding the large half. The form of vibration may possibly be held to indicate a complete closure of the cords whereby they actually touch each other. This is supposed to be a characteristic of spoken vowels as distinguished from sung vowels. The *a* sung by HERMANN[1] shows a gradual rise and fall of intensity such as would arise from a free vibration of the cords without touching of their edges. Spoken vowels, however, may be also produced by free vibrations of the cords as in the case of the *I* analyzed below (p. 25).

In this *I* there appears a trace of the strong secondary resonance vibration discussed below (p. 23); the phenomenon is here so faint that a discussion of it is best postponed to the study of the 2d example of *I*. The resonance tone indicated by it has a period of $3\frac{1}{2}^{\sigma}$, or a frequency of 286; this is approximately the note shown in Fig. 9.

FIG. 9.

The resonance vibration in the first part of the word has a period of 1^{σ} or a frequency of 1000. Its pitch is approximately as indicated in Fig. 10.

FIG. 10.

[1] HERMANN, *Phonophotographische Untersuchungen*, IV., *Untersuchungen mittels des neuen Edison'schen Phonographen*, Arch. f. d. ges. Physiol. (Pflüger), 1893 LIII Tafel II.
HERMANN, *Weitere Untersuchungen ü. d. Wesen d. Vocale*, Arch. f. d. ges. Physiol. (Pflüger), 1895 LXI Tafel V.

As the period of the cord tone becomes shorter, the number of resonance vibrations to each period becomes smaller. Beyond the 30th period of the cord tone the resonance vibrations show a lengthening of period. In the 39th cord vibration the resonance tone reaches a period of 2.2$^\sigma$ or a frequency of about 450; it thus falls more than an octave in the time of 9 cord vibrations, or, in this case, in 33$^\sigma$. Here the resonance tone is nearly but not quite of the same period as the octave, 2$^\sigma$, of the cord tone, 4$^\sigma$. This change is shown in the hand-drawing, Fig. 11, which be-

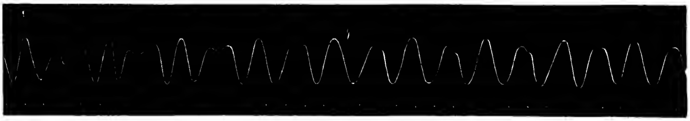


FIG. 11.

gins with the 31st vibration. This relation between resonance tone and cord tone is maintained to the end of the word; it produces the peculiar alternation of waves seen in the last two vibrations in Fig. 11.

The vibrations up to the 31st unquestionably belong to the *a*. In the vibrations beyond the 39th both the cord tone and the resonance tone are constant, except for a slight fall at the end. They unquestionably belong to the *i*. The vibrations from the 31st to the 39th show a constant cord tone and a falling resonance tone. They are presumably to be considered as belonging to the "glide." During the *a* the cords have been stretched more and more until at the 31st vibration they reach the tension required for the *i*; the only further change necessary is the lowering of the resonance tone.

Beyond the portion shown in Fig. 11 the curve shows strong vibrations so nearly alike that one is naturally induced to consider each one a cord vibration, as shown in Fig. 13. This would not be the proper way because close inspection shows that succeeding vibrations differ slightly, while alternate ones are alike. This likeness of all the resonance vibrations in the *i* as contrasted with the *a* is probably also due to a difference in the action of the cords; this difference appears more clearly in the word *eye* analyzed below, and the discussion is postponed to that point.

With the understanding that no definite limit can properly be made between one sound and the neighboring one in this case, we may, on account of the foregoing consideration, consider the *a* to have occupied the time 203$^\sigma$ ending with the 30th vibration, the glide to have occupied 33$^\sigma$ ending with the 38th vibration and the *i* to have occupied the remaining 216$^\sigma$.

The resonance tone of the *i* is one of about 450 vibrations per second, or about that in Fig. 12.

FIG. 12.

This resonance tone is much lower than the very high tone assigned to *i* by HERMANN and others but is not so low as those assigned by some other observers. There is, however, the possibility of different tones in the vowels from different speakers and also that of several resonances in the same vowel. In careful examination of the curves I find them often marked by small additional vibrations. These are frequently quite prominent

FIG. 13.

in the *i* of *ai*. Their fineness rendered it impossible to settle on any definite facts regarding them. In the drawing, Fig. 13, I have tried to give some idea of how the curve of the *i* might appear if freed from the defects of tracing. It is impossible to assign any period to these small vibrations; the regularity in the drawing was adopted for purely mechanical reasons.

The changes of the cord tone and the resonance tones are indicated in a general way in Fig. 14.

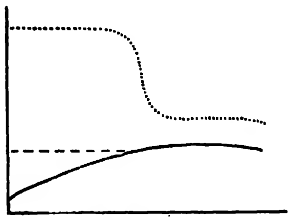


FIG. 14.

.... Upper resonance tone.
- - - - Lower resonance tone.
——— Cord tone.

Amplitude.—The amplitude of a vibration is the distance from the position of equilibrium to the extreme position on either side; it is thus one-half the difference in altitude between the crest and the trough of a wave. The course of change in amplitude is given in Fig. 15. The horizontal axis represents time as explained for Fig. 6. The vertical axis represents amplitude.

The initial resonance vibration of the first puff of this *a* has an amplitude of less than 0.1^{mm}. This slowly increases to 0.3^{mm} at the 20th vibration after which it remains practically constant to the 38th. Beyond

the 38th, that is, from the beginning of the *i*, the amplitude rapidly increases from 0.3^{mm} to 0.7^{mm} at the 50th vibration; thereafter it slowly sinks, becoming 0.3^{mm} at the 60th vibration and 0.2^{mm} at the 80th. 0.1^{mm}

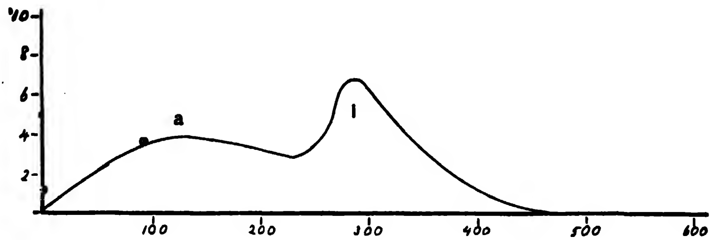


FIG. 15.

at the 88th and 0 at the 96th. The vibrations of the *i* just beyond the 50th, or the maximum of the *i*, are shown in Fig. 13; in this figure two of the large vibrations belong to one cord vibration.

The maximum for the *i* is 2⅓ times that for the *a*.

Ending.—The word *ai* ends by a gradual cessation of the expiratory impulse with hardly a noticeable change in the tension of the vocal cords; this is the clear ending usual in English. The slight fall in pitch of the *i* toward the end indicates a change that may be apparent in the auditory effect of the word, although it cannot be distinguished separately. It is probably due to a relaxation of the cords.

Relation between curve and color.—To the ear the sound of this word *I* appears from the record "colorless, without emotion, without inflectional rise or fall within the word, a monotone" (O.); "a mild statement" (E. W. S.).

The mildness of this word seems related to its length and its gradual changes in pitch and intensity.

ai in the word *I* (second example).

The second case of the word *I* occurs in the sentence *I killed Cock Robin.*

The complete reproduction of the curve is given in Fig. 16. The first five puffs are shown enlarged in the drawing, Fig. 17.

This word occupies an interval of 334^{σ}. It is preceded by a silent interval of 420^{σ}, or nearly half a second; this considerable interval would indicate a full stop. The words *With my bow and arrow* seem therefore in the thought of the speaker to belong to the previous *I*. The thought seems best indicated by a period after *arrow*; thus, *I, said the sparrow, with my bow and arrow. I killed*, etc. This second *I* is followed by

an interval of about 125^{σ} before any trace of the following sound can be found.

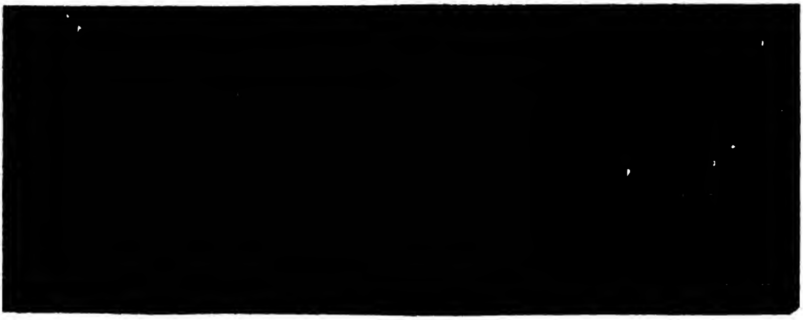


FIG. 16.

FIG. 17.

Beginning.—Similar to that of the 1st *I*, p. 16. The first five vibrations are shown in the drawing, Fig. 17.

Pitch.—Beginning with a period of 12^{σ}, the cord tone changes steadily through 9, 8, 8, 7, 7, 6, 6, 6, 6, 5, 5, 5, 5, 5, 5, 5, 4, 4, 4, 4, 4, 4, 4, etc., to the 48th vibration after which it slowly falls to 4.4^{σ} at the 70th. The course of the pitch-change is shown in Fig. 18; the plotting is done in the manner described for Fig. 6.

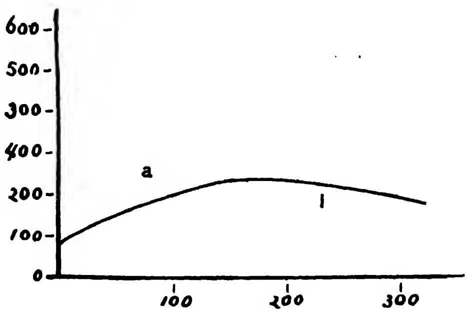


FIG. 18.

Formation.—The formation of the *a* is apparently the same as in the preceding case; the secondaries indicate a resonance tone of 1000, as in Fig. 10. At the distance of $3\frac{1}{2}^{\sigma}$ beyond the beginning of the vibration

there is another large oscillation markedly greater than the other secondaries, as shown in the drawing Fig. 18. This large secondary keeps at the same time behind the primary. As the pitch of the cord tone rises, the primary resonance vibrations come closer together; the large secondary, being at a constant interval behind the preceding primary, thus comes steadily closer to the following primary until it disappears in it. A drawing of two such vibrations is given in Fig. 19.

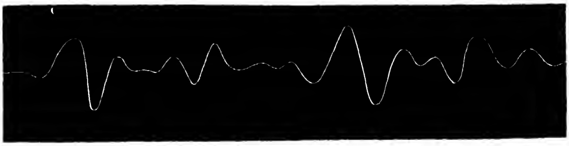


FIG. 19.

I do not believe that this larger secondary is due to an overtone-vibration of the cords. A stretched string or a reed may vibrate primarily as a whole, secondarily in halves, thirds, and so forth, producing the fundamental tone and its overtones. As the tension of the string or reed is increased, the fundamental tone rises in pitch and its overtones must do so likewise. For example, a string or a reed that vibrates in halves in addition to its fundamental vibration, will continue to vibrate in halves as the tension is changed. The curves for this vowel do not represent such a vibration. The strong secondary keeps at the same distance after the preceding primary while the distance to the following primary steadily decreases.

Two explanations of this phenomenon may be proposed.

It might be suggested that the primary and the strong secondary may represent two waves of a lower resonance while the primary and the other secondaries represent the waves of a higher resonance; this resonance would have a period of 3½ or a frequency of about 286. The note corresponding to this tone is shown in Fig. 9. It would require a rather large cavity to resonate to such a low tone. Such a cavity may perhaps arise from the pharynx and mouth acting as a single resonator of great length. There would then be at least three tones present in the *a*: the rising cord tone, the lower resonance tone of 286, which finally coincides with the cord tone, and the higher resonance tone of 1000.

Another explanation that may at least be considered is that the strong secondary arises from a flap-like action of the cords. The closure of the glottis across the air-current brings about a vibration of the edges, producing a tone whose pitch depends upon the tension of the edges. The edges can be assumed to vibrate as wholes in the manner of stretched

strings. As the tension is increased, the pitch rises. In addition to this the tissue stretching from the edges to the walls may also vibrate in unison with the edges, but just as in the case of a piece of cloth attached to a string, it may be assumed to execute an additional flap owing to the first impulse being reflected from the further walls to which the membrane is attached. If we assume that the tension of this tissue (Musculus thyreo-arytenoideus) remains constant during the vowel, this membranous flap would be independent of the tension of the cords and would follow it at a constant interval. This flap would impress itself with the air current and thus produce a stronger resonance vibration at a constant interval after the primary resonance vibration. On the assumption that the regular repetition of a sound produces a tone, the large secondary would combine with the preceding primary to produce a tone with a period of 3.5^σ or a frequency of about 286. Likewise it would combine with the following primary to produce a tone of changing pitch; this tone would start with a period of 5.6^σ or a frequency of about 178 and rise steadily in pitch till it disappeared.

The lowering of the resonance tone can be clearly seen at the 12th vibration just as at the 31st in the preceding case, although it may possibly begin earlier; it is finished at the 28th. Thus, 80^σ can be assigned as the time occupied by the *a*, 70^σ by the glide and 184^σ by the *i*.

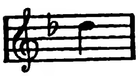


FIG. 20.

The resonance tone of the *i* has a period of 1.8^σ or a frequency of about 555; this is approximately the note shown in Fig. 20.

The smaller vibrations are also present as mentioned on p. 20.

The changes of the three tones in this vowel are indicated in Fig. 21.

Amplitude.—The maximum amplitude in the first vibration is less than 0.1^{mm}; it increases steadily to 0.4^{mm} at the end of the *a*.

Beyond the 25th vibration the amplitude begins to increase; it reaches a maximum of 0.6^{mm} at the 31st vibration. Thereafter it decreases rather rapidly, becoming 0.2^{mm} at the 45th vibration and fading away gradually to 0 after the 75th. If the vibrations from the 12th to the 30th are to be considered as the glide, the maximum occurs just after the beginning of the *i*.

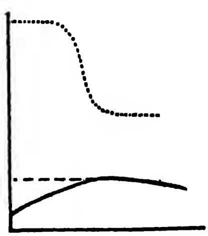


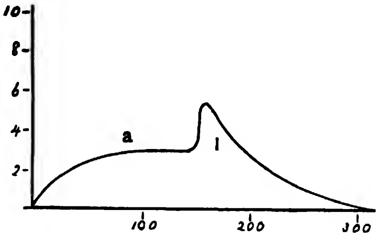
FIG. 21.

.... Upper resonance tone.
- - - - Lower resonance tone.
——— Cord tone.

The *i* is thus weaker throughout than in the previous case; its maximum amplitude is also slightly less. Owing to the loudness of the

a, the maximum amplitude of the *i* in this case is only 1½ times that of the *a*.

The course of change in amplitude is shown in Fig. 22; the plotting is done in the manner described for Fig. 15.



Ending.—Similar to that of the 1st *I*, p. 21.

Relation between curve and color.—To the ear this *I* is "shorter than the 1st *I*; more emphatic" (O.); "the word is spoken emphatically and boldly" (E. W. S.).

The emphatic character of the word may arise from its shortness, the loudness of the *a*, the quick fall of the *i*, or from other causes not determined.

ai in the word *I* (third example).

The third example of *I* occurs in the words *I, said the fly.*

This word occupies an interval of 598^{σ}. It is preceded by a long silent interval of 560^{σ}, or over ½ of a second, indicating the full stop after the question has been asked. It is followed by a silent interval of 200^{σ}, or $\frac{1}{5}$ of a second, indicated in print by a comma.

Beginning.—The first strong resonance vibration is preceded by 4 very small secondaries, Fig. 23. This would indicate that the expiration be-

FIG. 23.

gan before the cords had closed for their first explosion but that the mouth was already in position for the vowel. Such a brief passage of air through the mouth before the cords began to vibrate would cause the resonance tone to be heard for a brief instant before the cord tone began. In this case the resonance tone began 4 thousandths of a second before the cord tone. This can hardly be considered as an extremely brief

aspirate, or *h*; the time is too short, 5^{σ}, for any perception of the sound distinct from the rest of the vowel.

It is quite possible that this manner of beginning a vowel may be that called by ELLIS and SWEET a "gradual glottid" and by SIEVERS a "lightly breathed beginning." "In this the cord opening passes through the positions for toneless breath and whispering before the cord tone begins, whereas the really strong impulse of expiration begins only at the moment when the voice itself sounds."[1]

Pitch.—Beginning with a period of 7.7^{σ}(131 vibrations per second) it rises to 7^{σ} at the 8th vibration to 6^{σ} at the 13th, to 5^{σ} (200 vibrations)

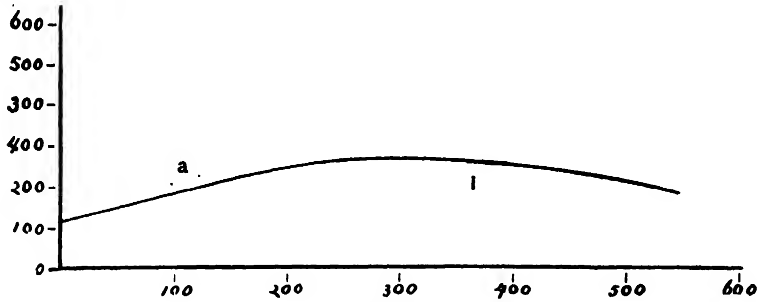


FIG. 24.

at the 20th, slowly to 3.8^{σ} (250 vibrations) at the 40th after which it remains constant to the 70th. Thereafter it falls slowly to 4.2^{σ} at the end. The course of change in pitch is shown in Fig. 24, which is plotted in the manner described for Fig. 6.

Formation.—The primary and secondary resonance vibrations are present in the *a* as in the previous cases but the secondary vibrations are relatively stronger in this case. This would indicate a more gradual opening of the cords; not so much of the energy of the puff is expended at the start, and some of it is reserved to carry the reasonance longer. There is no silent interval within the puff.

In the greater part of the curve the secondary vibrations in the *a* differ in form from those of the previous cases. They take a form that would indicate a series of partial tones differing from each other in phase by ¾ as shown in the drawing, Fig. 25.

Some of the curves for the other cases of *I* appear of the simple pendular harmonic form, but many of them show tendencies toward forms with the overtones differing in phase by ¼. Those that resemble the cases of difference by 0 and ½ cannot be distinguished from simple curves

[1] SIEVERS, *Grundzüge der Phonetik*, 4. Aufl., 140, Leipzig 1893.

on the small scale of the records. According to HERMANN the differences in phase produce no differences in the tone heard.[1] I note this particular vowel, however, as its curve differs from the others. The different forms

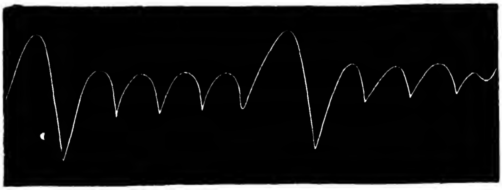


FIG. 25.

for different cases of *I* presumably indicate differences in the shape of the mouth.

The curve in this *a* presents great irregularities; they are all explainable, however, from the gradually rising pitch of the puffs whereby the number of resonance vibrations is gradually reduced as in the previous cases.

Just as in the previous cases the resonance tone begins to change while the cord tone is constant. The change begins somewhere around the 40th vibration and proceeds rather rapidly to the 50th. Thus 217^σ can be assigned to the *a*, 46 to the glide and 335^σ to the *i*.

The resonance tone for the *a* has, as before (p. 18), a period of 1 or a frequency of 1000 (Fig. 10). The resonance tone beyond the 50th vibration—which we may consider as the beginning of the *i*—has a period of 2.0^σ, or a frequency of 500, or approximately as indicated in Fig. 26.

FIG. 26.

The resonance tone remains constant for about 20 vibrations of the *i* and then slowly falls with the cord tone to about 2.2^σ at the end. The resonance tone of the *i* is very closely the octave of the cord tone.

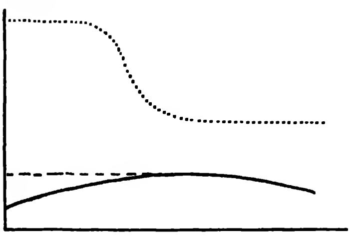


FIG. 27.

.... Upper resonance tone.
- - - - Lower resonance tone.
——— Cord tone.

The resonance vibrations of the *a* show a fairly strong secondary (p. 23) at 3.5^σ after the beginning. This would indicate a tone with a frequency of 286.

On the first hypothesis (p. 23) this would be the lower resonance tone, Fig. 9. On the second hypothesis it would be the constant flap tone; the changing flap tone would begin also with period of about 3.5^σ, and rise in pitch rapidly.

[1] HERMANN, *Beiträge zur Lehre v. d. Klangwahrnehmung*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LXV 467.

The changes in the tones of this vowel are indicated in Fig. 27.

Amplitude.—The amplitude of the maximum resonance vibration in the *a* is less than 0.1^{mm} in the first vibration; it gradually increases to 0.4^{mm} and remains constant to the end of the *a* and through the glide. After the glide the amplitude rises with moderate rapidity to 0.6^{mm} at the 62d vibration. Thereafter the amplitude falls more evenly and slowly to 0 than in the second example.

The course of change in amplitude is indicated in Fig. 28; the plotting is done in the manner described for Fig. 15.

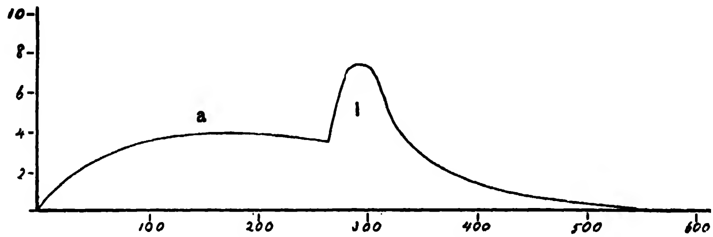


FIG. 28.

The amplitude of the *a* in this example closely resembles that in the 2d example; the *i* is also similar but its rise is more gradual and its fall more sudden. The amplitude throughout this example is a trifle less than in the first one. The maximum for the *i* is 1½ times that for the *a*.

Ending.—As on p. 21.

Relation between curve and color.—To the ear this *I* is "like the 2d but longer; a little more self-assertive" (O.); "spoken rather emphatically; like the 2d example rather than the first" (E. W. S.).

The maintenance of the pitch of the *i* may have something to do with this assertiveness.

ai in the word *I* (fourth example).

The fourth occurrence of *I* is in the line *I saw him die.* It occupies an interval of 350^{σ}; the word is thus shorter than any of the previous examples.

It is preceded by a silent interval of 165^{σ}, which is shorter than the similar interval before *I killed.* The speaker evidently feels that the words *With my little eye* belong to the following words *I saw* in making a sentence; thus no mark of punctuation should be placed after the word *eye.* This view is supported by the existence of a pause of 385^{σ} before the word *With.* In the previous stanza there was a pause of 770^{σ} after the words *With my bow and arrow* and of 0 (zero!) before them, that is, between *sparrow* and *with.* In that stanza the

speaker evidently felt the phrase beginning with *With* to belong to the preceding *I* and not to the following one. Both stanzas have been punctuated on p. 15 in accordance with these views.

The tracing of the *I* is followed by a straight line for 200^σ ; this time includes the pause after the *I* and the time of the *s* of *saw*.

Beginning.—The first primary resonance vibration of the *a* is preceded by several secondaries (see Fig. 30); the beginning thus resembles that of the 3d example, p. 25

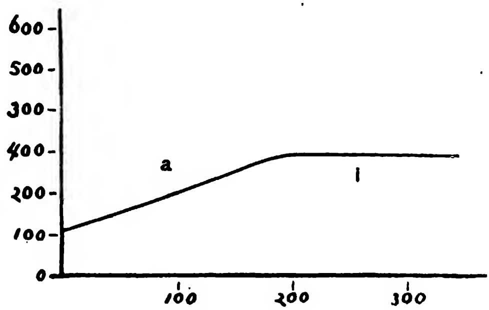


FIG. 29.

Pitch.—Beginning with a period of 9^σ it rises steadily through 8, 8, 7, 7, 7, 6, 6, 6, 6, 6, 6, 6, 5, 5, 4, 4, 4, ··· 4 (at the 28th), to 3½ at the 35th; this pitch is maintained practically unchanged to the end. In regard to pitch also this *a* closely resembles that of the 2d example but it is throughout a little higher. Starting with a frequency of about 111 it rises to about 286 and maintains this. The course of change in pitch is shown in Fig. 29, which is plotted in the manner described for Fig. 6.

Formation.—The first three vibrations are shown in the drawing Fig. 30. The motion of the cords is seen to be free and gradual as in the

FIG. 30.

third example, p. 26 and Fig. 23. The resonance vibrations in the *a* resemble those in the 2d example in having one of the secondaries stronger than the others. This secondary maintains its place in respect to the preceding primary resonance vibration with about 3.5^σ between them. As the puffs come more rapidly, the primaries come more closely in succession, cutting off the secondaries at the end in the usual way (p. 17). Thus the larger secondary comes steadily nearer to the following primary while maintaining its constant distance from the preceding primary.

If the primary resonance vibration and the strong secondary following it indicate a tone, the period of the tone will be about 3.5^σ and the frequency about 286. If a tone is to be considered as being formed by the interval from the strong secondary to the following primary, it would begin at about 4.5^σ, or a frequency of 220, and would rise in pitch till it is extinguished. In this respect this *a* closely resembles that in the 2d example of *I* (p. 23).

It is peculiar to this *I* that the cord tone rises during the *a* to the pitch of the lower resonance tone 286 and that the *i* keeps this pitch for the cord tone.

The upper resonance tone of the *a* has at the start a period of a little over 1^σ or a frequency a little less than 1000. The lowering of the resonance tone may begin at the start but it cannot be detected until about the 30th vibration, owing possibly to the unusual complexity of the curve in this case. Shortly before the 40th vibration it reaches 1.5^σ, and at about the 48th 1.8^σ. Around the 50th it reaches 2.1^σ, at the 65th about 2.5^σ; after this there is scarcely any fall to the end.

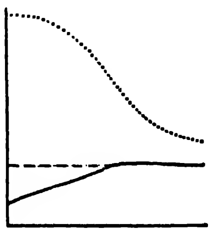


FIG. 31.

.... Upper resonance tone.
- - - - Lower resonance tone.
——— Cord tone.

The changes in the tones of this vowel are indicated in Fig. 31.

Amplitude.—The maximum amplitude in the first vibration is less than 0.1^{mm}; it increases rapidly to 0.3 in the 6th vibration, reaches 0.4½ at the 17th, decreases to 0.2½ at the 28th and remains with no noticeable variation from this till the 35th. In all previous cases the *a* has steadily increased in intensity; here we have a rise and a fall.

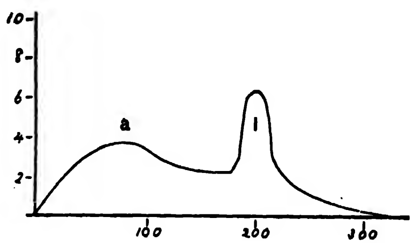


FIG. 32.

In the *i* the amplitude rises quickly from 0.3 to 0.7 at the 42d vibration of the word (7th of the *i*) after which it sinks quickly to 0.3 at the 45th and thereafter more slowly to the end. Such a quick fall of intensity is not found in any of the preceding cases of *i*. The loud part of the *i* is shorter than in the previous cases. The maximum amplitude is reached at its 13th vibration, where it is 1½ times that of the *a*.

The course of the change in amplitude is given in Fig. 32, which is plotted in the manner described for Fig. 15.

Ending.—The *i* ends with a steady fall in intensity without noticeable change in pitch.

Relation between curve and color.—To the ear the word seems to be spoken "like the 3d *I*" (O.); "triumphantly" (E. W. S.). The emphatic or triumphant character of the word may be due to its shortness. The high pitch of the word and the relation of tones arising from the strong secondary may also be elements tending to make the word emphatic.

ai in the word *I* (further examples).

Nine further cases of *I* were studied, making thirteen in all. In general the fundamental characteristics of the four cases already considered were found in all the rest. Some peculiarities, however, are to be noted.

Sometimes the first vibration of the *a* is shorter than the following one. This occurs, for example, in *I'll make his shroud*, and *I'll be the parson.* In the former case the periods are 9.8^{σ}, 11.6^{σ}, 10.9^{σ}, 9.8^{σ}, etc., and in the latter 8.1^{σ}, 10.5^{σ}, 9.8^{σ}, 8.8^{σ}, 8.8^{σ}, 8.1^{σ}, etc. The cords seem to receive an excess of tension before the breath begins and to be then relaxed to the tension desired. This suggests the possibility that in all cases of *I* the tension of the cords may be made greater than desired and that it is adjusted by relaxation before the breathing begins. There are two ways of reaching an adjustment of any muscular force, one by increasing the force upward until it reaches the proper point and the other by making an excessive increase and then relaxing. This latter method is familiar in many activities. I merely suggest its possibility in speech; I see no reason for supposing it to be the method employed in the cases of *I* that do not show it in the records.

Another peculiarity lies in the ending. Most cases of *i* in *I* fade slowly away in intensity while a slight fall in pitch takes place. In the case of *I* in *I caught his blood*, the vibrations reach a maximum in the early part of the *i* as usual and thereafter decrease in amplitude; but instead of steadily decreasing to zero they are rather suddenly cut off at a point 56^{σ} beyond the maximum, at which point the amplitude is about $\frac{1}{5}$ that of the maximum. Beyond this point there are still some faint vibrations in the tracing during a time of about 10^{σ}, after which the tracing is straight. The straight tracing represents the *k*-sound in the word *caught*; the faint vibrations correspond to the glide during which the cords are still vibrating but the mouth is changing from the *i*-position to the *k*-position. The condition seems to correspond to what may be called a "sharp cut off" to the vowel (KUDELKA: "stark geschnittener Accent"[1]) in contrast with the "fading end" to the cases of *I* above.

[1] SIEVERS, *Grundzüge der Phonetik*, 4. Aufl., 204 Leipzig 1893.

In the case of *I* in *I'll make his shroud* there is also no fading away; *i* passes into *'ll* and *'ll* into *m* without any break, although a fluctuation in amplitude takes place.

In one case the fall of the upper resonance tone appears to take place from the very beginning of the word; the resonance tone is thus steadily falling while the cord tone is steadily rising. This occurs in the *a* of *I* in *I, said the fish.* The period of the resonance tone begins with 1.4^σ, reaches 1.5^σ at about the 10th vibration, 1.8^σ at the 40th vibration and then remains constant to the end of the word. The typical *a* form is lost in the curve at this point, namely the 40th vibration, or 228^σ after the beginning; the typical *i* form appears clearly after the 45th vibration, justifying us presumably in assigning 19^σ to the glide and 240^σ to the *i.*

ai in the word *I* (prose example).

This occurs in the words *may, I can say, be summed up in a very few words* of the prose speech given on p. 15.

It occupies an interval of 354^σ. It is preceded by a silent interval of not over 16^σ; the preceding sound is *ay* of *may* which fades away slowly and may occupy in extreme faintness some of this interval. It is followed by a line showing no vibrations through an interval of 70^σ; this represents undoubtedly the gutteral *k* in the word *can* which seems to follow the *I* without pause as in the case mentioned on p. 31, yet the *k* does not cut off the *i* suddenly in this case as is shown by a study of the amplitude (p. 34 and Fig. 40).

Beginning.—Very faint but apparently clear, as on p. 16.

Pitch.—The successive periods are 9.8, 8.4, 7.0, 6.7, 6.0, 6.0, 6.3, 6.0, 6.0, 6.0, 6.0, 6.3, 6.7, 6.7, 6.7, 6.7, 6.3, 6.3, 6.3, 5.6 at the 20th

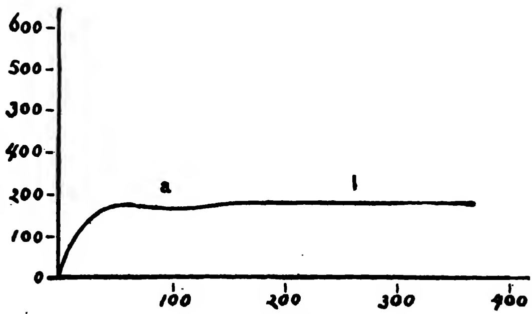


FIG. 33.

vibration; after this the period remains constant at 5.6^σ to the 68th vibration at the end of the word. This is indicated in Fig. 33, which is plotted in the manner described for Fig. 6.

Formation.—In general the curve resembles those with the strong secondary but with the difference that this secondary occurs at a smaller interval, 2.8^{σ}, after the primary. As the primary has a period of 1.0^{σ}, this produces the peculiar curve of which one vibration, is shown in the drawing, Fig. 34. This secondary is almost as strong as the primary in the early part of the *a*, but is lost sight of at a later point in the curve, possibly by coming into some relation to the upper resonance tone.

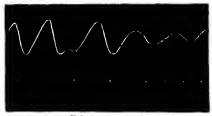


FIG. 34.

This difference from the previous cases would indicate some difference in the resonance adjustment of the mouth or in the action of the cords; it may possibly have something to do with the parenthetical character of the phrase.

The tone represented by the interval between the strong secondary and the preceding primary is constant at 2.8^{σ} or about 360, or approximately the note shown in Fig. 35. The resonance tone of the *a* starts at 1.0^{σ} or 1000, as in the first example, p. 18, being indicated approximately in Fig. 10.

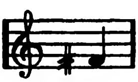


FIG. 35.

At about the 17th cord vibration the resonance tone begins to fall in pitch. As its period becomes longer, it more nearly coincides with the period between the strong secondary and the preceding primary; the curve becomes smoother and loses the little notch after the primary. The 20th vibration is shown in the drawing, Fig. 36. The resonance tone continues to fall slowly but steadily to the end of the *i*, reaching 2.8^{σ} or about 360 at the end; this is, curiously enough, the pitch of the lower resonance tone of the *a* (Fig. 35). The curve at the point where the *i* has fallen greatly in amplitude and the period of the resonance vibration is somewhat less than half that of the cord vibration is shown in the drawing, Fig. 37.

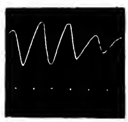


FIG. 36.

FIG. 37.

We are perhaps justified in placing the end of the *a* at the point where the resonance tone begins to fall, that is, at the 17th cord vibration; this would give *a* a length of 116^{σ}.

The vowel *i* thus continues the constant pitch of the *a* and also the

drop of the resonance tone in the glide. It is thus quite impossible to assign any limit between the glide and the *i*. Even the peculiar increase in amplitude that characterizes all the previous cases of *i* in *I* is here so gradual that it cannot be used to mark the limit (see Fig. 40).

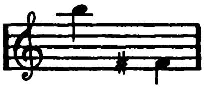


FIG. 38.

The remarkable fall of the resonance tone from 1.0$^{\sigma}$, or 1000, in the *a* throughout the *i* to its end at 2.8$^{\sigma}$, or 360, at the end, extends over about the musical interval of a duodecime, or approximately as indicated in Fig. 38.

The changes in the three tones of the *I* are indicated in Fig. 39.

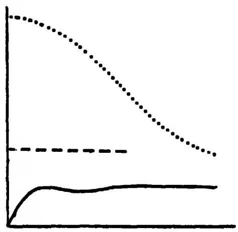


FIG. 39.

.... Upper resonance tone.
– – – – Lower resonance tone.
——— Cord tone.

Amplitude.—The amplitude increases rather steadily at first, then rapidly in the early part of the *i* and falls rather more rapidly than usual to the end. This is indicated in Fig. 40, which is plotted in the manner described for Fig. 15. The maximum amplitude for the *i* is about 1½ times that for the *a*.

Ending.—A fall of amplitude to o without any fall in pitch of the cord tone, as in the 4th example, p. 31.

Relation between curve and color.—To the ear this word is "colorless, unemphatic" (O.); "short, high, colorless, firm, a statement of no particular importance" (E. W. S.). It seems impossible to find any relation

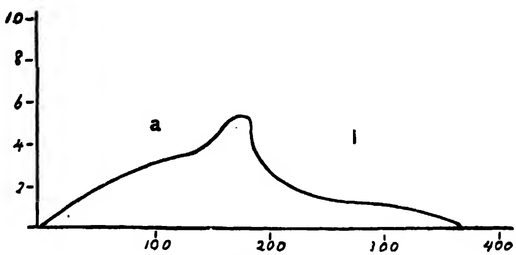


FIG. 40.

between these judgments and the recorded curve. Shortness was noted above (p. 31) as connected with emphasis; the unemphatic *I* (first example) was long and had a different curve of pitch. The very peculiar change in the resonance tone may by future collation with similar cases be found to be connected with the color of the word.

ai in the word *eye*.

The word occurs in the line *With my little eye*. A reproduction of the

curve is given in Fig. 41; the first few vibrations of the *a* are not very satisfactorily shown in the cut.

FIG. 41.

It occupies an interval 556^σ. It follows immediately on the last vibration of the *l* in the word *little*. The three words *my little eye* are here spoken with no separation. It is interesting, in passing, to consider the possibility that this fusion of the three words go parallel to a fusion of thought. It is evident from the very tone of the speaker that he is thinking of one thing, a certain *eye*, and that the facts of *mine* and *smallness* are not of any particular account to him.

The word *eye* is followed by a pause of 165^σ before the word *I* (see p. 28) which does not seem sufficient to justify a comma.

Beginning.—The faint vibrations of *l* in *little* die away just before the first primary resonance vibration of *eye* appears. The *a* begins as in *I*, 1st example, p. 16.

Pitch.—The vibrations of the preceding *l* decrease in amplitude until the line shows only a faint wavering. The first indication of *a* is a single resonance vibration on the line; this is repeated after 2.5^σ, and again after 3.9^σ. From this point the *a* curve clearly appears. It slowly falls in pitch to a period of 4.2^σ at the 20th vibration, 4.6^σ at the 40th, 4.9^σ at the 50th, 5.3^σ at the 54th, 5.6^σ at the 60th, 6.3^σ at the 66th, 6.7^σ at the 70th and 7.0^σ at the 73d. From this point onward the pitch continues to fall slowly, reaching 8.4^σ at the 80th vibration and ending with about 11^σ.

In pitch this *ai* differs radically from all the other examples; it starts with a moderately high pitch and falls continuously. The course of the change in pitch is indicated in Fig. 42, plotted in the manner described for Fig. 6.

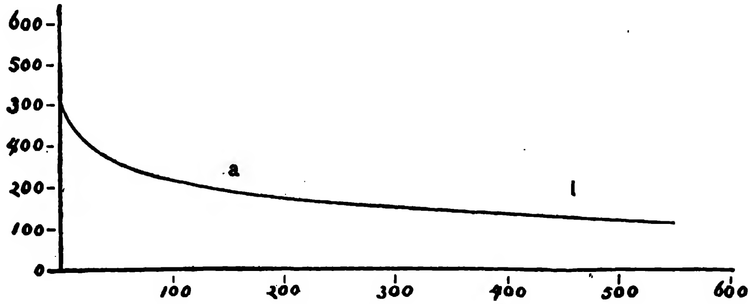


FIG. 42.

There is the possibility that the fall in pitch in this word may have something to do with its position at the end of a phrase. If the word had been followed by a long pause, it would naturally have fallen on account of its position at the end of a sentence; the pause, however, was extremely short and we cannot very well assume a short pause as the equivalent of a period unless we give up the accepted theory of relation between punctuation and time. It is, nevertheless, possible that this theory may have to be modified as later researches have shown that comma pauses may be long and semi-colon and colon pauses may be very short. I am inclined to think, however, that the true explanation is to be found by supposing the *ai* in *eye* to be a phonetically different sound from the *ai* in *I*, although the ear may not clearly distinguish between them. This point will be spoken of below in the section on general observations on *ai*.

Formation.—In the portion from the beginning to the 43d cord vibration the formation resembles that of the 2d and the prose examples of *I* in having a large secondary resonance vibration at a constant distance after the beginning of the primary one; this constant distance represents a period of 2.3^σ or a frequency of 435 (indicated in Fig. 43) as contrasted with the period of 3.5^σ (frequency of 286, Fig. 9) for the former and 2.8^σ (frequency of 360, Fig. 35) for the latter. After the 43d vibration there is a change in the curve indicating a change in this large secondary; apparently it decreases and disappears but I have not been able to decide with any confidence just what happens. After the 43d vibration the curve resembles that shown in Fig. 25.

FIG. 43.

The resonance tone of the *a* has a period of about 1^{σ} or a frequency of about 1000 (Fig. 10). At about the 40th cord vibration the period begins to lengthen, becoming 1.8^{σ} at the 63d, 2.1^{σ} at about the 77th, after which it continues to fall slowly to 2.5^{σ} at the end. The resonance tone of the *i* is thus on an average about the same as the lower resonance tone of the *a* (Fig. 43).

In spite of the fact that the fall in resonance begins at about the 40th vibration, the curve maintains its typical *a* form till after the 70th vibration. Beyond this point there is a decided difference, which is fairly well apparent in Fig. 41. The primary resonance vibration is of about the same amplitude as that of the *a* but the secondaries are all nearly as large as the primary. Such a difference might possibly be explained by a difference in the action of the vocal cords. The following theory is proposed. In the *a* they vibrate so that the air current is entirely cut off at one point in each vibration; the pressure of the air forces them outward suddenly, producing a strong puff after which there is an interval before the cords again strike and cut off the air. This puff sets the air in the resonance chamber into vibrations that decrease in amplitude. As long as this complete closure occurs, any increase in the force of expiration will increase the force of the puff and of the primary and secondary resonance vibrations in approximately the same ratios. Increased force will change the amplitudes without essentially modifying the original form of the curve.

During the *i* there is no such great predominance of one resonance vibration over the others; the secondary resonance vibrations are nearly as strong as the primary. This is the case also in all the examples of *ai* studied above, but here it is very striking on account of the fact that the cord period for the *i* is longer and not shorter than that for the *a*; there can thus be no attempt at explanation of the strength of the secondaries by the assumption of force gained by the shortening of the cord period. The explanation rather seems to lie in a different action of the cords. The following theory is suggested. In the formation of this *i* the cords do not strike or entirely close the air passage and thus the emission of air at the beginning is strong and steady rather than explosive; the first resonance vibration would thus be somewhat stronger than the following ones but all would be nearly alike. The increased force in the *i* would make all of them nearly as strong as the primary of the *a* as in this word, or even far stronger than in the cases of *I* studied above.

The changes within this word are so gradual that any assignment of definite limits for the *a* and the *i* would be apparently capricious. The distinct *a* character appears to my eye to be lost somewhere after the 66th

vibration and the distinct *i* character to begin somewhere about the 72d. If these points are selected as limits—an action that is hardly justifiable —the *a* would occupy an interval of 315^{σ}, the glide 35^{σ} and the *i* 206^{σ}. The *a* is at any rate longer than the *i*, in quite a marked opposition to the cases analyzed above.

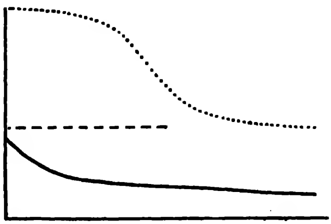


FIG. 44.
.... Upper resonance tone.
- - - - Lower resonance tone.
——— Cord tone.

The changes of the three tones in the *I* are indicated in Fig. 44.

Amplitude.—The *a* rises from zero as usual to an amplitude of 0.5^{mm} at the 17th vibration and remains practically constant to about the 66th vibration, after which there is a slow decrease to zero at the end. There is not the rapid increase to a maximum in the *i* found in the cases of *I* studied above. The maximum for the *i* is somewhat less than that for the *a*. The course of the change is indicated in Fig. 45, which is plotted in the manner described for Fig. 15.

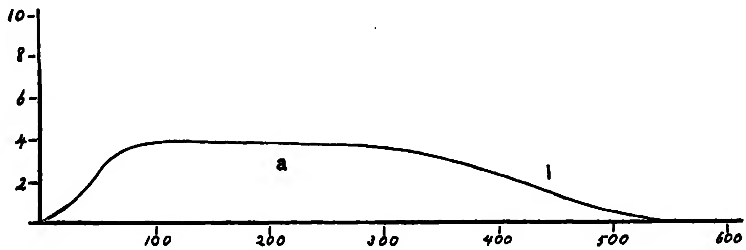


FIG. 45.

Ending.—This occurs by a fall of the amplitude to zero.

Relation between curve and color.—The ear notices that this word appears "weaker than the preceding *I*'s and also than the cases of *die*; lower in pitch" (O.); "somewhat higher in pitch than most of the *I*'s but not so high as the immediately following *I*; a somewhat colorless and unimportant word, differing quite from the modulated, flexible *fly* just preceding" (E. W. S.). The weakness of the word seems related to the falling pitch and the weakness of the *i*. The words *die* and *fly* are considered below. To the ear there is no essential difference between the *ai* in *I* and that in *eye*, yet the speaker makes a difference as indicated by the curves of results for pitch and amplitude.

ai in the word *die* (first example).

This occurs in the phrase *Who saw him die?* The word occupies an

interval of 510^{σ} of which 47^{σ} belong to *d* and 463^{σ} to *ai*. The curve of the entire word is reproduced in Fig. 46.

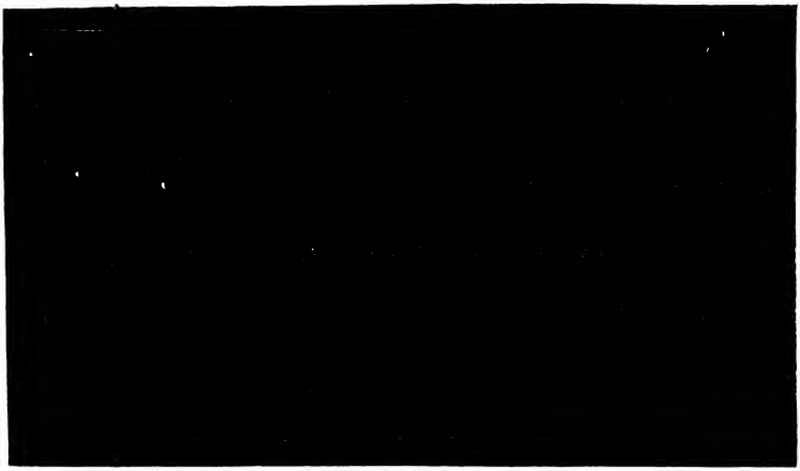


FIG. 46.

Beginning.—The word begins with 20 vibrations belonging to the *d*. These vibrations have a period of 2.0^{σ} or a frequency of 500. At the present moment it is impossible to say whether these are resonance vibrations imposed on a cord vibration or separate cord vibrations; it is quite probable that they are cord vibrations as they have no appearance of being grouped as is the case in resonance vibrations imposed on cord vibrations.

The amplitude increases rapidly from zero to 0.3^{mm} at the end of the *d*. Immediately after the strongest vibration of the *d* there follows a set of strong vibrations showing the *a* form.

In speaking the word *die* a decided movement of the larynx can be felt with the fingers; this would indicate a considerable difference between the tension of the cords for *d* and that for *a*. The period of this first vibration is 3.2^{σ}; its amplitude is 0.3^{mm}. The *a* thus begins promptly and loudly, as might be expected from the fact that the expiration is already in progress and the cords are already in vibration. The pitch of the *a* in the first vibration is higher than in the subsequent vibrations as might be expected on the assumption that the cords are already stretched to give a period of 2.0^{σ} for the *d*, and must be relaxed to produce the lower tone of the *a*. While this relaxation is going on, the cords must pass through all intermediate positions between that for a period of 2.0^{σ} and that for one of 3.2^{σ}. This occurs to a large extent apparently within

the time required for the vibrations of the *d*. At the same time the mouth is changing from the *d* position to the *a* position. These facts seem sufficient to explain the curve of change in the drawing, Fig. 47;

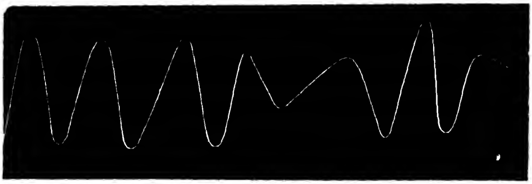


FIG. 47.

the three vibrations on the left are the last of the *d*, the strong one on the right is the primary resonance vibration of the first puff of the *a* and the connecting line shows the curve during the glide.

Pitch.—The successive periods of the cord vibrations are 2.8, 3.2, 4.9, 5.6, 5.3, 4.6, 4.4, 4.2, 4.2, 4.2, 4.2, 4.2, 4.1, 4.1, 4.0, 4.0, 4.0, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 4.1, 4.1, 4.2, 4.2, 4.2, 4.3, 4.3, 4.3, 4.4, 4.4, 4.5, 4.5, 4.6, 4.6, 4.6, 4.6, 4.6, 4.7, 4.7, 4.7, 4.7, 4.8, 4.9, 5.0, 5.1, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.5, 5.7, 5.9, 6.0, 6.1, 6.3, 6.5, 6.7, 7.0, 7.2, 7.4, 7.4, 7.5, 7.6, 7.7, 8.1, 8.4, 8.8, 8.9, 9.1, 9.5, 9.8, 10.5, 10.9, 11.2, 12.3, 13.0. These figures may be 0.1^{σ} either side of the correct values as, owing to instrumental difficulties, the curves could not be read to a smaller unit than 0.1^{mm}.

The pitch thus quickly descends from the tone of 500 vibrations for the *d* to one of 179, then ascends to one of 257 and then again descends slowly to the very low one of 77. These changes are shown in Fig. 48, which is plotted like Fig. 6.

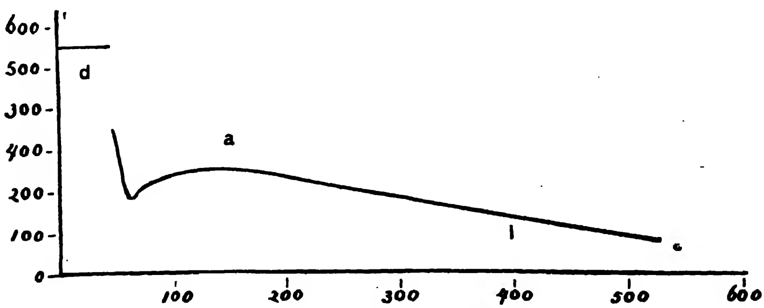


FIG. 48.

Formation.—The *a* portion of the curve resembles that of *I*, 1st example, p. 17. The resonance vibration in the first part has a period of

1^{σ} or a frequency of 1000, as in the first *I*, p. 18, Fig. 10. At about the 40th cord vibration it is lengthened to 1.4^{σ}, at the 55th to 1.6^{σ}, at the 58th to 1.8^{σ}; after this it changes slowly reaching 2.1^{σ} at the 75th and increasing but little more to the end at the 86th.

At about the 52d vibration the curve, while still retaining the *a* form, appears to begin to take on the *i* character as described on p. 19; the *i* character appears fairly complete at about the 57th vibration. Although no definite limits are to be made, we can assign very roughly 240^{σ} to the *a* and 220^{σ} to the *i*, or about half of the time to each.

No trace of a strong secondary resonance vibration in the *a* portion can be detected. The *a* starts at a pitch too high for the lower resonance tone found in the previous cases, but even after the pitch has fallen this tone seems to be absent.

A rather peculiar distribution of amplitude among the resonance vibrations can be seen in the *a* portion in Fig. 46. Although the puff for the cords is strong and sudden, as indicated by the large abrupt primary resonance, yet the force of the puff is not so quickly exhausted as in previous cases, as indicated by the greater size of the following resonance vibrations. The second case of *die* (below) resembles this one in this respect.

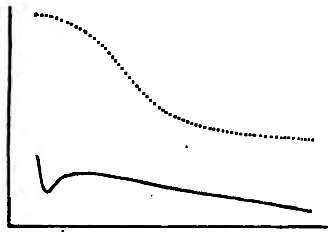


FIG. 49.

.... Resonance tone.

——— Cord tone.

The changes in pitch of the two tones of this *ai* are indicated in Fig. 49.

Amplitude.—The vibration begins with an amplitude of 0.3^{mm} for the primary resonance vibration which becomes 0.4^{mm} at about the 35th vibration; it sinks thereafter very slowly to zero at the end. The maximum amplitude is thus found in the *a* and there is no such sudden rise as is found in all the cases of *I* above. The course of change is indicated in Fig. 50 plotted like Fig. 15.

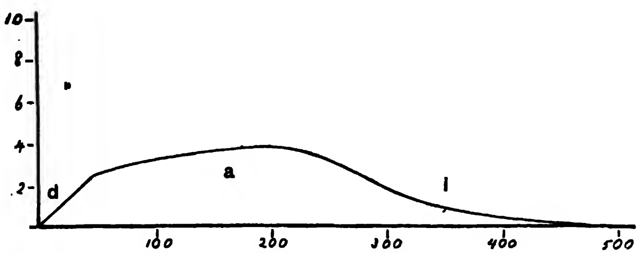


FIG. 50.

Ending.—The *i* ends with a fall in both pitch and amplitude, indicating simultaneous relaxation of the cords and the respiratory pressure.

Relation between curve and color.—The effect on the ear is that of "more emphasis at the beginning with decrease toward the end" (O. and E. W. S.). The high pitch of the *d* and the *a* at the start seem to correspond to the word-color.

ai in the word *die* (2d example).

This occurs in the phrase *I saw him die.* The entire word occupies an interval of 504^{σ}, of which 28^{σ} can be assigned to the *d* and 476^{σ} to the *ai.* The entire curve is reproduced in Fig. 51.

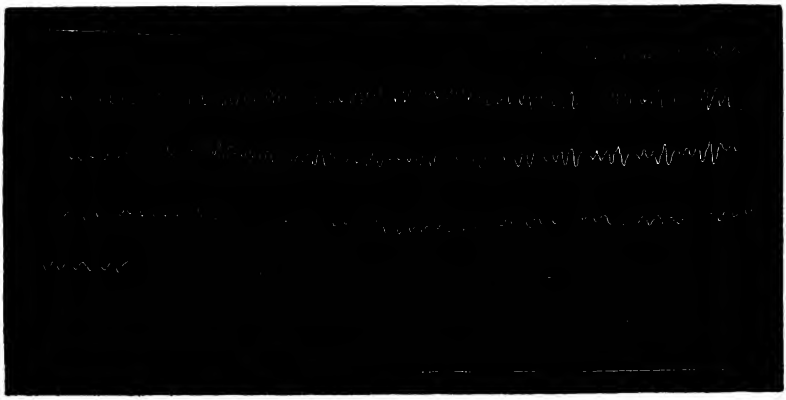


FIG. 51.

Beginning.—The word begins with 11 vibrations rapidly increasing in amplitude from 0 to 0.4^{m} and having a constant period of 2.5^{σ}, or frequency of 400. These are the vibrations for the *d;* they resemble those of *die*, 1st example, p. 39.

The sudden fall in pitch after the *d* is quite marked. The *d* curve is lost at once. The following interval of 7^{σ} can hardly be said to be the first vibration of *a* as its secondaries are very irregular in form; during this interval the mouth is changing from the *d* shape to the *a* shape. The peculiar form of the vibration is well shown in Fig. 51; the secondaries of the first few *a* vibrations are, however, slightly more prominent than in the original curve.

Pitch.—The successive vibrations of *ai* occupy periods measuring 8.4, 7.7, 4.6, 4.2, 4.2, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.9, 5.3, 5.3, 5.3, 4.9, 4.9, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.6, 5.6, 5.6, 6.0, 6.0, 6.3, 6.7, 6.3, 6.0, 6.3, 6.3, 6.7, 7.0, 7.0, 7.0, 7.0, 7.4, 7.7, 7.7, 7.7,

8.4, 8.4, 8.4, 9.0, 9.5, 10.5, 10.5, 10.5, 11.2, 11.6, 12.3, 12.3, 12.3, 13.0, 14.0, 14.0, 14.7, 15.8, 15.8, 15.8, ?. As previously explained p. 13, these figures may be in error by one or two tenths of a sigma, or in ten-thousandths of a second. The pitch of the cord tone thus descends as low as a frequency of 63. The general course of pitch is shown in Fig. 52 plotted like Fig. 6.

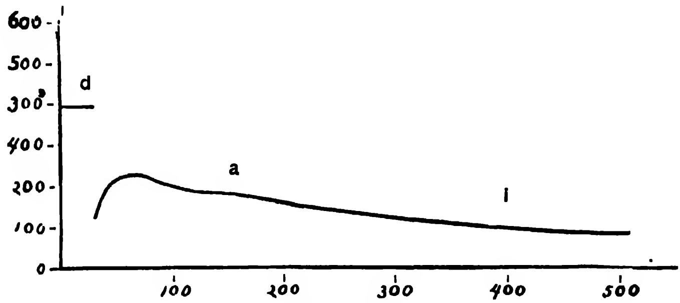


FIG. 52.

Formation.—The *a* curve differs from that of most cases of *ai* in having less difference between the first resonance vibration and the rest; the first and second are, in fact, of almost equal intensity. This would indicate a more gradual opening of the cords with less explosive effect. The *a* thus does not differ so much from the *i* as in most cases. Another case of *i* like this is found in the first example of *die* (above) and in *thy* (below).

The resonance vibration in the *a* has a period of 1^σ or a frequency of 1000 at the start (Fig. 10). It falls steadily, reaching a period of 1.4^σ around the 20th vibration, 1.8^σ around the 40th, and 2.1^σ around the 60th, which is maintained to the end. There is no indication of a lower resonance tone.

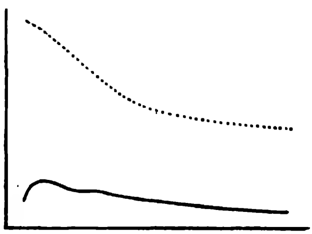


FIG. 53.

.... Resonance tone.

——— Cord tone.

The curve changes from the *a* form so gradually to the *i* form that it is quite impossible to place any dividing lines; each element of the diphthong may be said roughly to occupy half the total time.

The changes of the two tones are indicated in Fig. 53.

Amplitude.—The amplitude of the strongest resonance vibration begins at 0.3 and is maintained with fair consistency for about half the *ai*; after this it slowly falls to zero. This curve is given in Fig. 54 plotted like Fig. 15.

Ending.—The *i* ends with a fall in both pitch and amplitude, indicating a simultaneous relaxation of the cords and the respiratory pressure.

Relation between curve and color.—To the ear "it does not rise to a high pitch but starts with it and maintains it better than the other word

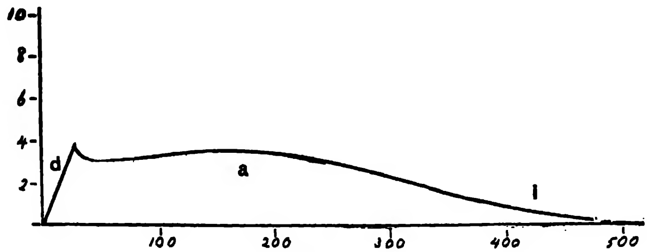


FIG. 54.

die" (O.); "it starts high and steadily falls" (E. W. S.). The apparent high start is probably due to the pitch of the *d.*

ai in the word *fly.*

This occurs in the phrase *I, said the fly.* The curve for *ly* occupies an

FIG. 55.

interval of 489^σ of which 25^σ belong presumably to the *l* and 464^σ to the *ai.* The curve is given in Fig. 55. It is followed by a silent interval of 371^σ which is longer than the comma pauses mentioned above (p. 16) and shorter than the full stop (pages 22, 25).

Beginning.—No specific details concerning the *f* can be derived from the curve. The strong vibrations just preceding those of the *a* are presumably from the *l* sound. They rise rapidly in intensity and greatly resemble those of the *d* in the two cases of *die* above; their period is 1.9^σ and their frequency 526.

Immediately after the last vibration of the *l* there follows a short *a* vibration with primary resonance vibrations not so strong as in the following ones. The cord adjustment seems not to be perfected for the *a* till the second characteristic *a* vibration occurs; this is well shown in Fig. 55.

The *a* begins promptly and loudly after the *l*.

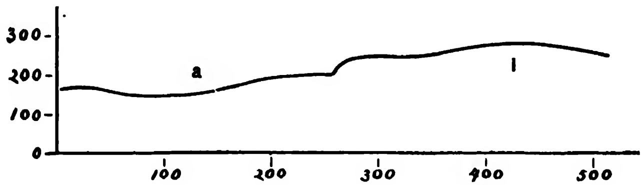


Fig. 56.

Pitch.—The successive periods of the cord vibrations are 6.0, 6.3, 6.3, 6.3, 6.3, 6.3, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.7, 6.3, 6.3, 6.3, 6.0, 6.0, 6.0, 6.0, 6.0, 5.8, 5.8, 5.8, 5.8, 5.6, 5.6, 5.6, 5.4, 5.3, 5.3, 5.3, 5.3, 4.9, 4.9, 4.9, 4.9, 4.9, 4.9, 4.9, 4.9, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.5, . . . (retaining this period for 27 vibrations) . . ., 3.9, 4.2, 4.2, 4.6, 4.6, 4.6, 4.6, 4.6. There is a rather sudden, though small, change in period from 4.9 to 4.2; this occurs at the irregular place a little to the left of the middle of the fourth line of the curve in Fig. 55. This is due presumably to a rather sudden tightening of the cords for the *i*. The course of change in pitch is shown in Fig. 56, which is plotted like Fig. 6.

Fig. 57.

Formation.—The *a* portion of the curve resembles that of the 2d example of *I* (p. 22), with a specially strong secondary resonance vibration at 3.9^σ after the primary, representing a tone with a frequency of 256. This is lower than in any of the previous cases (Fig. 57).

Fig. 58.

The resonance tone begins with a period of 1.6^σ or a frequency of about 625 (approximately as in Fig. 58). This falls slowly reaching 1.8^σ at about the 35th vibration, 2.0 at about the 40th, 2.2 at about the 50th, and 2.5 at the end. This indicates a resonance tone about the same as that of *i* in *eye* (p. 36, Fig. 43).

The change from *a* to *i* proceeds in general as in all the other cases but the change in curve-form seems a little more marked. It may be said to occur at the 43d vibration, or 301^{σ} after the beginning and 163^{σ} before the end.

The changes in the three tones are indicated in Fig. 59.

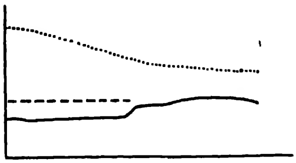


FIG. 59.

.... Upper resonance tone.

- - - - Lower resonance tone.

——— Cord tone.

Amplitude.—The *a* begins with an amplitude of 0.2^{mm} for the strong resonance vibration in the first puff from the cords, 0.3^{mm} for that in the second puff and rises quickly to $0.3\frac{1}{2}^{mm}$. After remaining fairly constant for a while, it becomes 0.3^{mm} toward the end of the *a*. In the first part of the *i* it rises to 0.4^{mm}, after which it gradually falls to zero at the end. The change of amplitude shown is in Fig. 60, which is plotted like Fig. 15.

Ending.—The *i* ends, like most of the cases examined, in a combined fall in pitch and intensity.

Relation between curve and color.—To the ear this word has a fall-and-rise of intonation like that of *well* and *yes* in such dubitative as *Well, you may do so if you wish, but I would prefer not. Yes, it may very well be true although we have no evidence for it* (O. and E. W. S.).

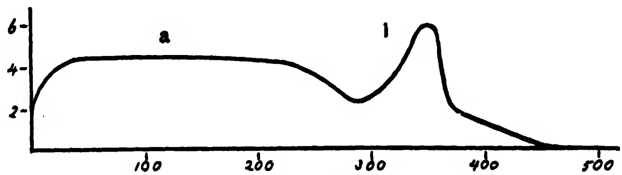


FIG. 60.

The word *fly* appears to sink and then rise in intonation to a greater degree than the corresponding words *sparrow*, *fish*, etc. This fall-and-rise is due to the fall from a tone of the frequency 526 of the *l* to one of 160 at the beginning of the *a* and then the rise in the *a* and *i* as shown in Fig. 56. Probably the reason for the rise in the *i* is to be found the rising intonation usual in English at the end of a parenthetical clause;[1] that the clause *said the fly* is such a one inserted in the statement *I, with my little eye, I saw him die* seems indicated also by the fact that the silent interval after *fly* is less than that usual for a period. If *said the fly* were not parenthetical, there would probably be a longer pause

[1] SWEET, New English Grammar, § 1946, Oxford 1898.

after *fly* and it would have a falling instead of a rising intonation. In this case the lines would read: *Who saw him die? I, said the fly`. With my little eye I saw him die.* If special weight is to be given to the falling intonation of *eye* (p. 36) as opposed to the brevity of the pause after it, then *eye* would be considered as ending a phrase. The reading required by the intonations of *fly* and *eye* would thus be: *Who saw him die? I, said the fly', with my little eye`. I saw him die.*

ai in the word *thy* (first example).

The word occurs in the phrase *Hallowed be thy name* on the gramophone record plate described on p. 15. Much of the work on this word has been done by Miss E. M. COMSTOCK. The entire curve is reproduced in Fig. 61.

The time occupied by the word is 505.8^{σ}. It is preceded by a silent interval of 73.5^{σ}. It is followed by an interval of 145.3^{σ} before any trace of the *n* of the following word appears.

FIG. 61.

Beginning.—The word begins with 7 vibrations belonging to the *th*. These vibrations have a period of 2.5^{σ} or a frequency of 400. These are probably cord vibrations for the same reasons as given in the case of *d* in *die*, p. 39. The amplitude increases rapidly from zero to 0.2^{mm} at the end.

Immediately after the last vibration of *th* there follows the first strong vibration of the set showing the *a* form. The beginning of the *a* is thus prompt and loud.

Pitch.—The successive periods of the cord vibrations in the *ai* are 7.0, 7.0, 7.4, 7.0, 6.7, 6.7 which is maintained with slight fluctuations to the end of the word. The sudden lengthening of the cord period

(that is, the lowering of pitch) at the start is peculiar; it is made specially so because it is accompanied by a sudden rise in the pitch of the resonance tone (see below).

Formation.—The vowel portion of the curve shows throughout its whole length a common character. This character is that of a group of resonance vibrations imposed on each of a series of cord vibrations. In the earlier portion these resonance vibrations are not of equal amplitude while in the later portion they are very nearly so. In the earlier portion there is a strong primary resonance vibration followed by three secondary resonance vibrations (making a total of four resonance vibrations) except in the first two cord periods where there are only two secondaries after the primary (making a total of three). This first portion of the curve resembles that of an *a* but differs in having less difference between the primary and the secondary resonance vibrations; in this fact it resembles the typical *i*.

At the 40th vibration the number of resonance vibrations has changed from four to three, showing a strong initial vibration followed by decreasing ones with a pause before the initial vibration of the next puff. The typical *a* of the preceding examples appears here strongly.

The *i* vibrations may be said to begin in the 44th with three resonance vibrations of almost equal strength, the initial vibration being slightly the stronger.

In the latter portion there are 3 resonance vibrations to every cord vibration; the curve is that of a weak *i* of the kind seen in *eye*, p. 37.

If these vibrations just mentioned, namely the 40th and the 44th, may be considered as limits, the *a* may be said to occupy an interval of 258.3^{σ}, the glide an interval of 19.6 and the *i* an interval of 210.0^{σ}. This subdivision, however, is rather a questionable procedure.

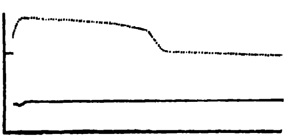


FIG. 62.

...... Resonance tone.

——— Cord tone.

The resonance tone in the first portion begins with a period of 2.1^{σ} or a frequency of 476 which immediately rises to 1.7^{σ} or a frequency of about 588 in the third vibration. The period then changes steadily to 1.9^{σ} at the 40th vibration; it becomes 2.4^{σ} at about the 44th and remains constant to the end. The sudden rise of the resonance tone at the start is accompanied by an equally sudden fall of the cord tone (see above). It seems tnaural to infer that the resonance cavity of the mouth for the *d* must have been lower than that required for the *a*.

There is no trace of a lower resonance tone as described on p. 23.

Fig. 63.

The changes in the tones of *thy* are sketched in Fig. 62. In general the resonance tone of the *a* can be said to be one of 588 vibrations or approximately as indicated in Fig. 63, and that of the *i* to be of 416 vibrations or as in Fig. 64.

Fig. 64.

Amplitude.—The primary resonance vibration on the first cord vibration of the *ai* has an amplitude of $0.1\frac{1}{2}^{mm}$. Up to about the 50th cord vibration the amplitude fluctuates between 0.1^{mm} and 0.2^{mm}; after that it gradually falls to zero. The fluctuations may be due to interference of the resonance vibrations. The course of amplitude is indicated in Fig. 65 which is a sketch and not a careful plot like Fig. 15.

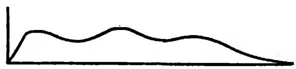


Fig. 65.

Ending.—The word ends by a fall of intensity with maintenance of the cord tension (p. 31).

Relation between curve and color.—The sound of this word *thy* as taken from the record appears to the ear "higher and shorter than the second example; varying more in pitch, rising rapidly at first and then falling" (E. M. C.); "high and short when compared with the second one" (E. W. S.).

The measured results show a shorter word of higher pitch than the second example. There is a slight rise at the start but no fall. The following word *name* is much lower in pitch.

ai in the word *thy* (second example).

The second example of *thy* occurs in the phrase *Thy kingdom come.* A reproduction of the curve is given in Fig. 66. Most of the work on this word has been done by Miss E. M. Comstock.

The curve for this word shows 6 faint vibrations at the beginning. These belong presumably to the *th* and correspond to the strong vibrations of *th* in the first *thy*, and of *d* in *die.* In contrast with the cases just mentioned these vibrations are so weak that little can be said about them definitely except that their period is 2.4^{σ}. It is just possible that they may belong to the first cord vibration of the *a*; this is suggested by the fact that the period is the same as that of the resonance tone of the *a.* Although the matter is doubtful, we have assigned the beginning of the *a* to the end of these vibrations.

The *ai* in this word occupies an interval of 1085^{σ}. It is preceded by a silent interval of 2100^{σ}, represented by a period and including possibly a short time for the *th.* It is followed by a silent interval of 324^{σ} which undoubtedly represents the gutteral *k.*

Pitch.—Begining with a period of 11.9^{σ} the cord tone changes slowly, reaching 8.4 at the 10th vibration, 7.7 at the 20th, 7.4 at the 30th, and 7.0 at the 60th, which it maintains to the end.

Formation.—The curve of the *a* differs from most of the cases of the *ai* studied above in regard to the resonance vibrations. The first resonance vibration for each cord vibration is followed by a second one nearly as strong and this by a third one somewhat weaker, whereas in the previous cases there was one resonance vibration greatly exceeding the rest in amplitude. The curve suggests a more gradual opening

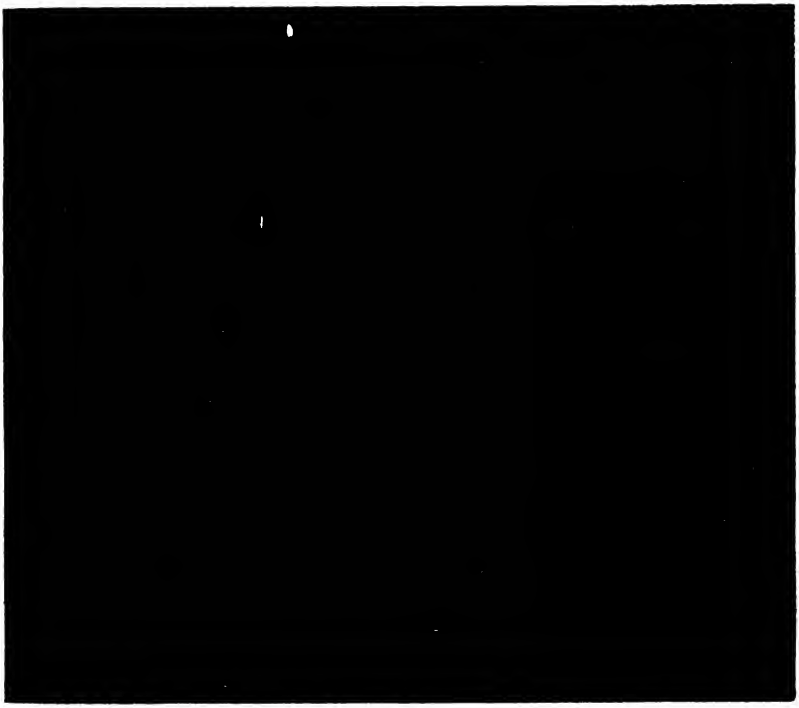


FIG. 66.

of the cords and a less explosive effect; the cord action in this *a* may be supposed to somewhat resemble that in the *i* as explained on p. 37.

There is no strong secondary of the kind described on p. 23. The *a* thus resembles the *a* in *die* and *thy* (above) rather than that in *I* in showing no evidence of a strong lower resonance tone.

The resonance vibration in the first part of the word shows a period of 2.4^{σ}. It rises to 1.6 at the 20th cord vibration falls to 1.9 at the 50th, 2.2 at the 70th, 2.5 at the 90th, and 3.5 at the end. This curious

rise of the resonance vibration during the *a* has not been observed in any of the previous cases. The rise and fall are so gradual that it is impossible to decide on any place as the turning point between them. For the same reason it is impossible to divide the word into *a*, glide and *i*. In the earlier portion the typical *a* form is distinctly seen in the curve and in the latter portion the typical *i* form, but the main portion shows a gradual passage from the former to the latter. There is no sudden increase in amplitude as in nearly all the *i*'s studied.

The changes in the two tones of *ai* are indicated in Fig. 67. It will be noticed that the resonance tone of the *a* begins on the same pitch as the tone of the *d*.

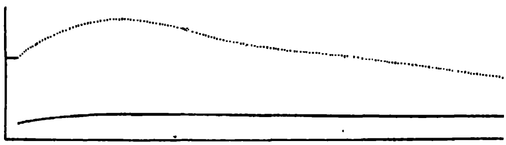


FIG. 67.
...... Resonance tone.
——— Cord tone.

Amplitude.—The amplitude runs from 0.1^{mm} in the first part of the word to 0.2^{mm} at the 30th vibration, falls to 0.1^{mm} at the 50th, increases to 2.5^{mm} at the 70th, maintains this figure to the 80th and gradually falls to zero. The change in amplitude is indicated in Fig. 68.

Ending.—The sound *ai* ends by a fall of amplitude, the respiratory pressure gradually ceasing while the cords are still tense.

Relation between curve and color.—To the ear this word "is longer and more mellow than the first example" (E.M.C.); "begins low and rises with considerable inflection as compared with the first example" (E.W.S.).

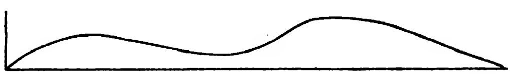


FIG. 68.

The measured results show a very long word, beginning very low and rising in pitch.

General observations on *ai*.

The *ai* in the cases studied above is to be considered as a union of two speech sounds, that is, as a diphthong.

The family of sounds represented by *ai* contains many members that differ greatly in their characters. This is true of the *same speaker on a*

single occasion; the changes for different speakers and for the same speaker on different occasions may be left out of consideration at present.

The first sound in *ai* in words like *fly, my, thy,* etc., is generally stated to be an *a* (as in *ah*) which inclines toward the mixed *œ*, that is, the vowel sound heard in *burn* and *about;*[1] it may even shade into the palatal *æ* (as in *man, fat*)[2] while in some cases it has a tendency to broadening, even to *o* (*not*) as in the Irish.[3] These statements all refer to British forms of pronunciation.

The second sound in *ai* as in *fly* is said to be a very open *i*, something between the *i* in *kin* and the *e* in *ken.*[4]

The diphthong *ai* cannot contain the vowel *ī* as in *keen* or *i* as in *kin*. By holding down the tongue and lower jaw with a pencil it is not possible to pronounce either *keen* or *kin*, whereas there is no difficulty in saying *I*. It seems rather to be the vowel sound heard in the last syllable of *foxes*.

The sounds given above as the British pronunciation of *ai* do not, to my ear, correctly represent the North Atlantic form as heard in the region around New York. In this speech the first sound of *ai* seems to be a somewhat short *a* (as in *father*). Both pronunciation and curve indicate it to be like the *a* in *parson*, below. A similar judgment by the ear has been given by GRANDGENT.[5]

In its first half the North Atlantic *ai* (as in *I, eye*) seems to resemble the average German *ai* with a distinct *a* (*father*) sound. The second half seems to be different in the two cases. For the sake of comparison several cases of *ai* were examined in some records which were traced off with the machine described on p. 10 but with a shorter recording lever. Various words like *ein, weisser, Eis, Zeiten, Schein,* etc., were closely studied in the tracings from Record No. 1500, *Die Lorelei and Der Fichtenbaum*, by W. L. ELTERICH. When examined under the magnifying glass, the *a* portion of the record showed in most cases curves analogous to those in the cases of *I*, whereas the *i* portion was extremely weak. This peculiarity of the weak *i* in the German *ai* and the very strong *i* in

[1] VIETOR, Elemente d. Phonetik, 3. Aufl., 95, 101, Leipzig 1894.
SWEET, Handbook of Phonetics, 9, Oxford 1877.
STORM, Englische Philologie, 2. Aufl., 358, Leipzig 1892.

[2] STORM, Englische Philologie, 2. Aufl., 142, Leipzig 1892.
LLOYD, *Speech sounds; their nature and causation*, Phonet. Studien, 1892 V 263; also a review on p. 87 of the same volume.

[3] SWEET, History of English Sounds, 21, Oxford 1888.

[4] VIETOR, Elemente der Phonetik, 3. Aufl., 95, Leipzig 1894.
STORM, Englische Philologie, 2. Aufl., 103, 358, Leipzig 1892.
LLOYD, Review in Phonet. Studien, 1892 V 87.

[5] GRANDGENT, *English in America*, D. neueren Sprachen, 1895 II 446.

most cases of the American *ai* gives the former the effect of containing a longer *a*. It must be noted, however, that many sounds usually treated as the same are really different. Thus the vowel in *weiss* in *Ich weiss nicht was soll es bedeuten* gives a curve differing greatly in character from that of *weisser* and the other words mentioned above. Again, some of the cases of the American *ai* reported above show a weakening of the *i* that indicates a tendency toward the German form. The details of the work now being done on the German *ai* will appear on a future occasion.

It has been pointed out that the quality of *ai* is different in a strongly accented syllable from what it is in a less accented one, as can be readily heard by comparing the two *ai*'s in *likewise*.[1] This difference is perhaps analogous to that found to exist between *I* and the words *eye*, *die*, *fly*, and *thy*.

The two chief sounds of *ai* are generally said to be joined by a rapid glide, which is not acoustically of much effect except to produce the impression of continuity.[2] Yet it has been asserted that such a diphthong consists in an even and gradual change of the vowel from beginning to end.[3] The above analyses show that the *ai* is not the sum of the two vowels *a* and *i* but an organic union into a new sound *ai*. Thus, there is no necessary pause or sudden change of intensity or change in pitch or even change in character. The later sound shows its influence in the earlier one, and the earlier one keeps its influence far into the later one. This is what would be expected on psychological grounds. The speaker does not think and speak of two sounds separately but of only one; the execution of this one idea by two distinct processes would be unusual. The various degrees of perfection of the synthesis of the two elements would correspond to various expressive characters of the resulting sound.

The degree of synthesis of the two elements would be lessened by any great or sudden change in intensity, pitch or character of the cord tone or the resonance tone. In some of the cases of *ai* there are greater changes than in others.

In so far as they can be considered to be constant, the resonance tones in these cases of the *a* and the *i* were found to be as in Table I. These results may be compared with those of other observers; this is done in Table II.

[1] Bell, Visible Speech, 113, London 1867.
Sweet, Primer of Phonetics, 76, §204, Oxford 1890.
Storm, Englische Philologie, 2. Aufl., 358, 405, 424, Leipzig 1892.

[2] Lloyd, Review in Phonet. Studien, 1892 V 83.
Storm, Englische Philologie, 2. Aufl., 204, Leipzig 1892.

[3] Soames, Introduction to Phonetics, 53, London 1891.

TABLE I.

a

	Lower resonance tone.	Upper resonance tone.
I, 1st example	286	1000
I, 2d "	286	1000
I, 3d "	286	1000
I, 4th "	286	1000
I (*I caught his blood*)	385	1000
I, prose example	360	1000
Eye,	435	1000
Die, 1st example		1000
Die, 2d "		1000
Fly,	256	625
Thy, 1st example		588
Thy, 2d "		416

i

I, 1st example	450
I, 2d "	555
I, 3d "	500
I, 4th "	400
I, prose "	360
Eye	400
Die, 1st example	473
Die, 2d "	473
Fly	400
Thy, 1st example	416
Thy, 2d "	288

When allowance for the individualities of different speakers is made, the two resonance tones that I have found for the *a* agree quite well with the tones found by other observers. The serious differences among these observers can be partially explained on the supposition that some have found the lower tone and some the upper one.

Although the *i* in *ai* is not the ordinary long *i*, its resonance tone shows some agreement with those of a few observers. The higher resonance tone noted by other observers was also probably present in the *i* but it was impossible to measure it in the examples studied above (p. 20).

Particular emphasis must be laid on the fact that the tones in a vowel are not constant factors and that the changes they undergo from instant to instant are presumably highly important in producing its peculiar character. Only two previous investigators have observed the change in the cord tone and no one seems to have suspected a possible change in the resonance tone.

TABLE II.

a

a in	Lower resonance tone.		Upper resonance tone.
I (E. W. S.)	d^1		b^2
I (E. W. S.)	$f^1\#$, g^1		b^2
Eye (E. W. S.)	a^1		b^2
Die (E. W. S.)			b^2
Fly (E. W. S.)	c^1		$e^2\flat$
Thy (E. W. S.)		d^2	
Thy (E. W. S.)		$g^1\#$	
a (WILLIS)		$d^3\flat$, f^3	
a (DONDERS)		b^1	
a (HELMHOLTZ)		d^3	
a (KOENIG)		b^2	
a (AUERBACH)		a^2, f^2, b^2	
a (TRAUTMANN)		f^3, g^3	
a (PIPPING)		$c^3\#$-d^3	
a (HERMANN)		f^2, g^2	
a (STORM)		$c^1\#$, d^1, $f^1\#$, a^1	
a (BOEKE)		$f^2\#$, c^3	
ai (BOEKE)		b^3	
a (BEVIER)	d^2–$g^2\#$		b^2–e^3

i

i in	
I (E. W. S.)	$f^1\#$ to $d^2\flat$
Eye (E. W. S.)	$g^1\#$
Die (E. W. S.)	$b^1\flat$
Fly (E. W. S.)	$g^1\#$
Thy (E. W. S.)	$a^1\flat$, ?
i (DONDERS)	f^3
i (HELMHOLTZ)	$f + d^4$
i (KOENIG)	b^4
i (AUERBACH)	c^3, f^1
i (TRAUTMANN)	f^4, g^4
i (PIPPING)	$\left\{\begin{array}{l} c^1\text{–}d^1 \\ c^4\text{–}d^4 \end{array}\right.$
i (HERMANN)	$c^4\#$–g^4
i (STORM)	d^2
i (LLOYD)	$\left\{\begin{array}{l} b^{-1} + a^4 \\ b^{-1} + d^5 \end{array}\right.$

The rise of pitch in the cord tone of the vowel *a* has been observed by BOEKE[1] to have extended over more than half a tone in words like *Vader*

[1] BOEKE, *Mikroskopische Phonogrammstudien*, Arch. f. d. ges. Physiol. (Pflüger), 1891 L 301.

(Dutch). MARICHELLE makes the following observations on his phonograph records of the vowel *a* sung on different notes. "The periods corresponding to low tones are divided into two distinct parts; the intensity is feebler in the second half of the period. The gradual modification of the character [timbre] under the influence of variations of pitch operates almost entirely at the expense of the less intense portion of the period; this second half even disappears little by little."[1]

I have observed similar changes in the *a* of the German *ei* and in the vowels *u* and *a* described below.

It seems hardly possible at the present moment to specify the positions of the mouth corresponding to the resonance tones and their changes. Some idea of them may perhaps be obtained in the following way. GRANDGENT'S sections[2] of the mouth for the vowels *a* and *i* are shown in Figs. 69 and 70.

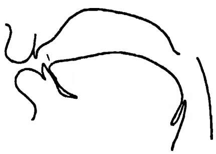


FIG. 69.

FIG. 70.

The following view of the physiological action of the vocal cavities in producing *ai* in the case studied above is proposed tentatively. The depressed position of the tongue for the *a* leaves open a large cavity reaching from the teeth to the vocal cords; the uvula offers no great interruption. The lower resonance tone of the *a* may be considered to arise from the vibration in this cavity. The upper resonance tone of the *a* may be supposed to arise from the rear resonance cavity, that is of the throat cavity from the cords to the slight elevation of the tongue at the uvula. As the *a* changes to *i* this elevation of the tongue moves forward enlarging the rear cavity by including continually more of the mouth; this continuously lowers the upper resonance tone until the tongue comes to rest in the typical *i* position. The variety of changes in the course of the upper resonance tone corresponds to individualities of action of the tongue in the various cases. In some cases the change from *a* to *i* is more sudden and definite (Figs. 14, 21, 27, 44, 62) and in others it is less definite (Figs. 31, 39, 49, 53); in other cases there is even laxity and fluctuation in the typical terminal positions (Fig. 66).

The supposition that the upper resonance tone arises from the cavity

[1] MARICHELLE, La parole d'après le tracé du phonographe, 47, Paris 1897.

[2] GRANDGENT, *Vowel measurements*, Publ. Mod. Lang. Ass., 1890 V 148.

behind the elevation of the tongue rather than from the one in front of it, although opposed to the usual view, does not exclude the presence of tones from the front cavity also. In fact these other tones are presumably also present though not distinguishable in my records.

The greater importance of the rear cavity seems to be indicated by the following facts. The laying of the finger on the tongue does not appreciably modify the enunciation of *a*. When the finger is introduced into the mouth and kept in front of the elevation for the *i*, it produces no appreciable effect; but when it is pushed beyond the elevation into the rear cavity it changes the sound completely.

It may be noted that curious relations exist between the tones of two succeeding sonants (speech sounds with tones); in general it is true that the tones of a sonant form approximately musical intervals with a tone or tones of the preceding sonant.

In all cases of *ai* there is no sudden jump of the cord tone; the *i* continues the cord tone of the *a*, forming with it the easiest musical interval, a unison. This tone is, however, different in different cases; the cord tone of the *a* rises to a certain point selected for that of the *i*. The selection of the pitch of the cord tone for the *i* is influenced by the preceding resonance tones of the *a*, as may be seen in the following table.

	Tone of	*Tones of the a.*				*Tones of the i.*	
	d, th, l.	Cord,		Lower	Upper		
		start.	end.	resonance.	resonance.	Cord.	Resonance
I, 1st example		56	250	286	1000	250	450
I, 2d "		83	250	286	1000	250	555
I, 3d "		131	250	286	1000	250	500
I, 4th "		111	286	286	1000	286	400
I, prose "		102	180	360	1000	180	360
Eye,		400	160	435	1000	160	476
Die, 1st example	500	179	200		1000	200	473
Die, 2d "	400	217	133		1000	133	473
Fly,	526	160	204	256	625	256	500
Thy, 1st example	400	143	149	588		149	416
Thy, 2d "	417	84	143	416		143	288

In the 1st *I* the cord tone of *i* is practically identical with the lower resonance tone of the *a*; the fixed lower resonance tone of the *a* apparently furnishes a standard toward which the cord tone of the *a* rises to begin the *i*. The cord tone is also just two octaves below the upper resonance tone of the *a*. There is no very simple relation between the resonance tone of the *i* and any of the tones of the *a*.

In the 2d *I* the relations are similar to those in the 1st *I*.

In the 3d *I* the cord tone of the *i* is also practically in unison with the lower resonance tone of the *a* and also at two octaves below the upper resonance tone. The resonance tone of the *i* is just an octave below the upper one of the *a*.

In the 4th *I* the relations are practically as in the previous one except for the fact that the resonance tone of the *i* is two and a-half octaves below the upper resonance tone of the *a*.

In the prose *I* the cord tone of the *i* is an octave below the lower resonance tone of the *a* while the resonance tone of the *i* appears as a continuation of the lower resonance tone of the *a* with no simple relation to its upper resonance tone.

In *eye* the cord tone of the *i* is one and a-half octaves below the lower resonance of the *a* and the resonance tone is practically a continuation of that tone, with no relation to the upper resonance of the *a*.

In *die* (1st example) the cord tone is five octaves below the upper resonance tone of the *a*, which has no lower resonance tone. It is also two and a-half octaves below the tone of the *d*. The resonance tone of the *i* shows no relation to any tones of the *a*, although it approximates the tone of the *d*.

In *die* (2d example) the cord tone of the *a* starts approximately an octave below that of the *d*. No other relations between the various tones are apparent.

In *fly* the lower resonance tone of the *a* is an octave below the tone of the *l*. The cord tone of the *i* in its main portion continues the lower resonance tone.

In *thy* (1st example) the cord tone of the *i* is approximately four octaves below the resonance tone of the *a* and its resonance tone is approximately in unison with the tone of *th*.

In *thy* (2d example) the resonance tone of the *a* is in unison with the tone of *th*. The cord tone of the *i* is three octaves below this tone. The resonance tone of the *i* is an octave above its cord tone and 1½ octaves below the resonance tone of the *a*.

Such a relation between successive tones in speech is merely what would be expected in a melodious voice. An illustration of a similar relation will be found below in the account of the sound *ll* of the word *who'll*.

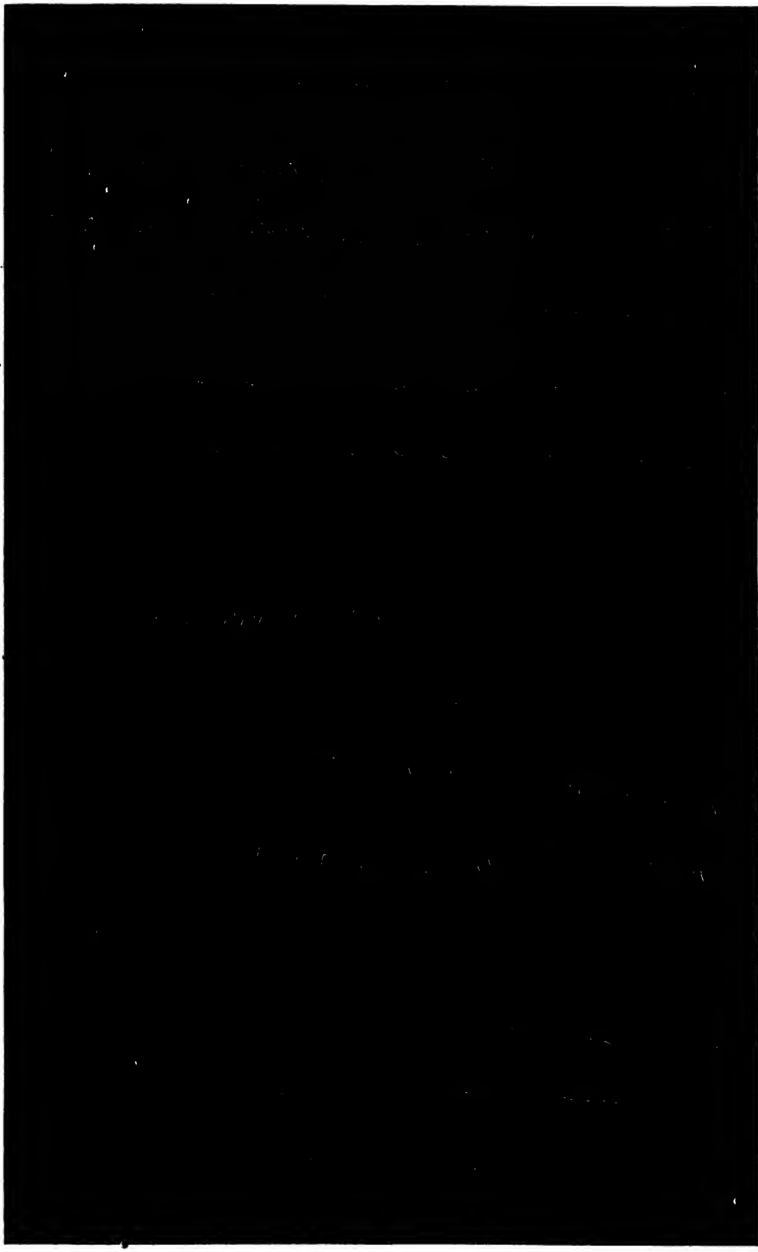
Study of the words "*Who'll be the parson?*"

In the following phonetic analysis of a complete phrase I have been much assisted by Miss E. M. Comstock.

The complete curve is given in Fig. 71. It begins with the breath

FIG. 71.

1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17



h u u l h i i dh z p a a r sn n n

indicated by the letters *wh*; this is not the sound of *wh* in *which* but the breathing *h*.

The aspirate *h*.

The first of the series of sounds is heard as an aspirate followed by the vowel *u*. The curve (Fig. 71, line 1) shows that it occupies a time of 35^{σ}. Its tone has a constant period of 2.8^{σ}, or a pitch of about 380 vibrations per second. Its amplitude rises from zero to a maximum of 0.2^{m}. It is thus a "crescendo sustained" sound; in particular, a crescendo sustained light breath. The tone of the *h*, as shown by the vibrations in the curve, is a resonance tone arising from the passage of the air through the mouth; it is not a cord tone. The reasons for considering the vibrations to have arisen from a resonance tone and not from a cord tone are the following: (1) such high cord tones are not found in the other sounds produced by this speaker; (2) the vibrations of 2.5^{σ} are followed by two vibrations of 2.3^{σ} and 2.1^{σ} respectively and then by the vibrations of the *u* beginning with a cord tone of 6.3^{σ} and a resonance tone of 1.9^{σ}; the tone of 2.5^{σ} thus leads rather to the resonance tone of the *u* and could with hardly any possibility be considered as a cord tone with an instantaneous drop of three octaves.

These results do not agree with the view that the first sound of *who'll* is a voiceless form of *u*. The sound *h* is usually said to arise from the breath passing through the mouth already adjusted to the following vowel, the cords being open and the resonance tone alone being heard. "In the opinion of some authorities, *h* has the same position as the beginning of the following vowel."[1] Most later observers have adopted the same view.[2] According to this view we cannot speak of a single *h* but must suppose for each vowel a corresponding *h*: h^{a}, h^{i}, h^{u}, h^{e}, h^{o}, etc., each of which has an adjustment of the mouth like that of the corresponding vowel and differs from that vowel only in having a noise from the cords instead of a tone.[3] "Our *h* combines a noise from the cords (and subsidiarly a noise in the mouth-cavity) with the mouth position of a vowel. The common element in h^{a}, h^{i}, h^{u}, etc., does not lie in the vocalic position of the mouth-cavity, which is really different, but in the larynx at the vocal cords, whose position is here a peculiar one, different from

[1] Tâittirîya Prâtiçâkhya, ii 47, ed. by WHITNEY, Journ. Amer. Oriental Soc., 1871 IX 77.

[2] MICHAELIS, *Ueber das H und die verwandten Laute*, Arch. f. d. Studium d. neueren Sprachen (Herrig), 1887 LXXIX 49, 283.

[3] See quotations in MICHAELIS, as before, 79.

that for the loud voice (*vox*) and from that for the whisper voice (*vox clandestina*)."[1]

A special adjustment of the mouth for *h* seems to have been first asserted by VALENTIN who remarks: "The palate, apparently narrowed as a whole, is somewhat drawn upward whereas the root of the tongue is moderately arched."[2] MERKEL asserts: "The whole cavity from larynx to mouth-opening opens or narrows itself at once to the degree required by the following vowel. The tongue however in forming the *h* does not yet assume the position required for the vowel in question. Thus when *i* is to follow it lies lower than the position for this vowel."[3] Both these and a series of later observers apparently supposed the configuration of the mouth to aid in the rough noise of the *h*. This view is undoubtedly partially true as in many cases of *h* the friction of the air can be felt in the mouth. I venture to suggest, however, that the assumption of a particular position for the *h* is for the purpose of giving it a resonance tone instead of making more noise by friction; the curve for *wh* in the case under consideration shows a resonance period of 2.5^{σ} as contrasted with that of 1.9^{σ} for the following *u*.

Indications of a tendency to give *h* an independent resonance cavity are apparent in remarks by LLOYD. "*h* and *o* in *hold* are successive, but they slightly overlap. When such a combination is to be produced, the cords instantly leap into a position sufficiently close to cause a slight friction. They then close more slowly, until they are planted close together, and voice ensues. The vowel *position* has already been assumed, but there is no vowel so long as the glottal orifice is still comparatively wide. But there is a moment, just before the cords begin to sound, when the glottis is narrowed to a whispering position; and, for that moment, the sound is both *h* and whispered vowel. If *ho* is whispered, the *h* is still prior, for it begins with a glottal orifice so large as quite to mar the adjusted resonance of the *o* vowel-configuration; and there is no vowel until the close position of whisper is reached. When that is reached, it is held; and the whispered vowel itself may be viewed as the mere promulgation of the final element of the *h*. *h* is therefore really a glide from simple *Mund und Kehlresonanz* (such as is heard in a *sigh*) to a whispered *Anlaut* of the following vowel, *i. e.*, from a nearly uniform beginning to a far from uniform end."[4]

[1] MICHAELIS, as before, 79.

[2] VALENTIN, Lehrbuch der Physiologie des Menschen, II 291, 1844, quoted by MICHAELIS, as before, 61.

[3] MERKEL, Laletik, 72, 1866, quoted by MICHAELIS, as before, 72.

[4] VIETOR, Elemente der Phonetik, 3. Aufl., 22, Leipzig 1894.

There seems to be some conflict between LLOYD's statement that in the *h* the vowel position has been already assumed and that it starts from a nearly uniform beginning. I would suggest the view that the *h* in this case of *wh* possesses a definite resonance cavity of its own which may be related to but is yet different from that of the following *u*.

The most plausible view of the nature of this *wh* seems to include the following points.

In the first place it is either the glottal fricative produced by a narrowing of the glottal opening sufficient to produce a rough sound without a tone, or a sonant fricative produced by a narrowing of the proper cord glottis while the cartilage glottis remains open.[1] Both views are consistent with the fact that a distinct movement of the larynx can be felt with the fingers when *wh* is pronounced. The former view is consistent with the curve under consideration, but the latter view is favored by some of the other cases of *wh* in the record studied, which show some slight but not quite certain indications of a grouping of the resonance vibrations and therefore of the presence of a cord tone.

The *h* is considered as a sonant in all Sanskirt treatises.[2] Traces of a sonant *h* have been found in speech curves of the Finnish language. The consideration of the vexed question of sonant *h* must be postponed to a future occasion.

In the second place the *h* contains at least one tone arising from the resonance cavity in front of the cords. This tone I believe to be one of a pitch peculiar to *h*, just as certain tones are peculiar to certain vowels. The frequency of the *h* tone in this *h* is 400. I do not believe that for this tone the mouth is adjusted to the position of the following *u* with a resonance tone of 526, and that the pitch of the cavity is modified by the difference in the greater enlargement of the glottal orifice so that the tone 400 is produced. My reasons for this last statement are: 1st, *h* can be sounded alone without giving information concerning the following vowel; 2d, the difference between the opening of the cords for the *h* position and that for the vowel position is too small to produce such a great difference in the pitch of the resonance cavity; 3d, the assumption that the *h* cavity is the same as that of the following vowel is not supported by any positive proof and in the absence of such proof it is unwise to accept an

[1] CZERMAK, *Ueber d. Spiritus asper und lenis, etc.*, Sitz.-Ber. d. Wiener Akad., math.-naturw. Cl., 1866 LII (2) 630, Anmerk. 1 (also in Schriften, I 756).

[2] Tâittirîya Prâtiçâkhya, i 13.

[3] PIPPING, *Zur Phonetik d. finnischen Sprache; Untersuchungen mit Hensen's Sprachzeichner*, Mém. de la Société finno-ougrienne, XIV, Helsingfors 1800. (Review in Deutsche Litteraturzeitung, 1900, April 28.)

arrangement involving an anticipatory adjustment of the vocal organs whereby the vowel is prepared for before the *h* is produced.

The *hu* glide.

The aspirate *h* is followed by two vibrations with periods of 2.3^σ and 2.1^σ respectively (Fig. 71, line 2). They are resonance vibrations produced by the passage of the air through the mouth cavity. They might with propriety be considered as belonging to the *h*, from which they differ only in period. Yet the change from the *h* period of 2.5^σ denotes the rise of an impulse toward another sound and, if the concept of a glide is to be admitted at all, they are to be treated as a glide. The intention shown in the glide is to change the mouth tone from 2.5^σ for the *h* to 1.9^σ for the *u*. The second of these glide vibrations ends suddenly with the puff of air from the first vibration of the cords in making the *u*.

The vowel *u*.

The word is so short that the ear is not able to attribute any particular quality to the vowel.

The curve for the *u* (Fig. 71, lines 2 and 3) closely resembles that for *ai* in its general character. The first part shows a rising cord tone and a nearly constant but afterwards falling resonance tone. In the latter portion the cord tone is approximately constant while the resonance tone falls. The change in the character of the action of the cords appears clearly also as in *ai* (p. 37). It is, in fact, very evident that this sound is really a diphthong with possibly less difference between the two elements than in the case of *ai*. This diphthongal character of the English *u* is well known to phoneticians; the sound is generally indicated by *uw*. A separation of the sound into its two parts will not be attempted here.

The curve at the beginning of the *u* shows a vibration of 6.3^σ from the vocal cords acting on a cavity whose period 1.9^σ is not a sub-multiple of the cord period. As the cord period is gradually shortened, the resonance period (remaining the same) steadily modifies the form of the resultant vibration, and the curve is seen to change its form gradually. The relation between cord tone and resonance tone is closely analogous to that in the *a* of *ai* (p. 19).

The successive vibrations of the *u* occupy the periods of 6.3, 6.1, 6.1, 5.6, 5.4, 5.4, 4.9, 4.9, 4.9, 4.9, 4.9, 4.6, 4.6, 4.6, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.2, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6, 4.6^σ. The total time occupied by the *u* is 167^σ.

The *u* thus shows a sudden tightening of the cords to a tension necessary for a tone with a period of 6.3^σ and thereafter a gradual increase of

tension to a maximum represented by 4.2^σ, after which there is a fall to 4.6^σ at which the tone remains constant.

The resonance tone begins with period of 1.9^σ or a frequency of 526. For the vowel *u* the following resonance tones have been assigned: DONDERS, f^1; HELMHOLTZ, f; KOENIG, b; AUERBACH, g–b, f^1; TRAUTMANN, f^2, g^2; PIPPING, $f\sharp$–$f\sharp^1$, g^2–b^2; HERMANN, c^2–e^2; STORM, a; BOEKE, d^4. My measurements indicate a resonance tone of 526 vibrations a second, or approximately c^2. I have not yet been able to settle the question of a lower resonance tone.

This resonance tone is, however, not constant. This is especially evident during the last part of the *u* where the cord tone is constant. In this region of constancy the curve steadily changes its form from the earlier *u* form toward the *l* form; during the last 8 or 10 cord vibrations it is difficult to say whether the curve belongs to the *u* or the *l*. The cord vibrations of the *u* period persist in their own constant period, however, to a point which can be detected. We are thus justified in reckoning these vibrations to the *u* although the mouth cavity has been presumably steadily shaping itself for another sound.

Repeatedly observed facts of this kind have forced upon me the belief that the view of a word as composed of a set of fixed sounds with glides between them is a somewhat inadequate one. It is derived from the attempt to get away from the artificial character of spelling but it still largely retains that character. The usual view of the word *who'll* would represent it as composed of *h*—glide—*u*—glide—*l*. The vocal organs are supposed to occupy three distinct positions, the glides representing the intermediate positions during the moments of change.

A somewhat different view seems better fitted to the actual curves. The unit of speech is sometimes a phrase, sometimes a word, and never a vowel or a consonant unless it is at the same time a word. In speaking a word the vocal organs pass through a series of positions of a special character without stopping in any one position. Thus the word *who'll* represents a continuous change in the force of expiration following a definite plan, also a continuous change in the tension of the vocal cords, likewise continuous movements of the parts of the mouth. The force of expiration rises from 0 to a maximum in 35^σ at the end of the *h*, continues with slight fluctuation during 171^σ in the glide and *u*, and finally dies away at 277 with the end of the *l*. Before the breath begins the mouth has adjusted itself to a tone of a period of 2.8^σ; this position changes very slightly during the 35^σ of *h*; then it makes a rapid change through 2.3, 2.1 to 1.9^σ in the *u*, remains constant during 167^σ and rises suddenly to the mouth tone of the *l* (not determinable here).

On speaking the word *who'll* I perceive apparently *continuous* movements of the lips and tongue; they do not assume fixed positions at any moment. This would agree with the changes just described.

The cord tone has a somewhat similar history. It begins with a period of 6.3^{σ} in the *u* at 39^{σ} after the beginning of the word; it rises steadily to 4.2^{σ} and then falls to a constant pitch of 4.6^{σ} for the latter part of the *u*; suddenly it rises to 2.1^{σ} for the *l* and remains practically constant for 71^{σ}.

There are thus at least three distinct but coöperating continuous processes following different courses throughout the word, namely, the force of expiration, the resonance tone and the cord tone.

It seems thus somewhat artificial to divide the word *who'll* into 3 or 5 sounds; we may preferably say that for the sake of discussion 5 stages in the changing sound may be picked out as typical of the whole process. To illustrate by an analogy, we might take single pictures out of a series of views of a runner made for the kinetoscope and treat the whole movement as made up of a series of positions in which the runner remains at rest. This treatment has its advantages for certain cases but we should never lose sight of the fact that the true movement occurs otherwise.

This view is not inconsistent with the fact that some of the elements of a vocal sound may remain approximately constant for a short time. Thus, the pitch of the *h* is nearly constant—as far as our methods can discover—though the intensity is changing, and the pitch of the *u* is fairly constant for a while.

The liquid *ll*.

The sound *ll* apparently does not begin suddenly but arises from a modification of the *u*. The *u* itself has been steadily changing its character from the very beginning; during its last five or more cord vibrations it gradually approaches the form of curve that characterizes the *ll*. After this point the curve takes the *ll* form which differs completely from that of the *u* at the start (Fig. 71, line 4). As stated above, the explanation is presumably (1) that the cord tone remains on the *u* pitch until a certain moment at which it suddenly rises to the *l* pitch, whereas (2) the mouth cavity begins to modify itself from the *u* form to the *l* form before the cord tone changes. This is quite in agreement with the view that in the English *l* the back part of the tongue is elevated whereby it receives a guttural character[1] and is in this respect related to *u*.

The *l* shows 34 vibrations with a constant period of 2.1^{σ}. It occupies a total time of 71^{σ}.

[1] Literature in STORM, Engl. Philologie, 2. Aufl., 139, Leipzig, 1892.

The form of the vibration steadily changes as shown in the figure.

The changes in pitch in this word *who'll* confirm the law deduced for *ai* (p. 57) to the effect that in a succession of sonants (speech elements with tones) the cord tone of a sonant tends to be a multiple or a sub-multiple of the cord tone or the mouth tone of the preceding sonant.

The relations are not exact but only approximate. The mouth tone 2.5^σ of the *h* is followed by a cord tone for the *u* having a general average of 5.0^σ or an octave below the former. The mouth tone of the *u* 1.9^σ is followed by a cord tone for the *l* of pretty nearly the same period 2.1^σ.

Such a law is what would be expected in a voice—at any rate in one that was not unpleasant—for the human ear finds pleasure in a succession of tones whose periods stand in certain relations. Possibly some of the explanation of disagreeable voices may be found in the violation of this law.

In general the curve of this *l* may be said to resemble the forms given by WENDELER[1] and HERMANN and MATTHIAS.[2]

The *l* given by WENDELER is a spoken sound; the figure shows that it must have had a falling cord tone and a decreasing intensity.

The examples of *l* studied by HERMANN and MATTHIAS were sung on notes of different pitch. Their analysis showed that these examples all contained a tone between f^3 and g^3. They also found for the lower notes also a tone that was the octave of the cord tone and changed with it, and for the higher notes a reinforcement of the cord tone itself. This reinforcement of a partial tone of the cord tone is not found in the vowels studied by HERMANN or in the cases of *ai* considered above except in two cases, namely, in the *i* in the 3d example of *I* and in *fly* (see list on p. 57). There is apparently some difference in the action of the mouth in forming the *l*. This difference may be felt by singing the *l* on a note of rapidly rising or falling pitch; there is apparently a movement of the tongue whereby it is pressed more strongly against the palate as the pitch rises. The consequent change in the size of the resonance cavity might, by the appropriate connection between tongue and cord, go parallel with the change in the cord tone and thus always reinforce one of its partials.

Our curve does not enable us to make any measurements of the resonance tones, but its steady change in form while the cord tone re-

[1] WENDELER, *Ein Versuch, die Schallbewegung einiger Consonanten und anderer Geräusche mit dem Hensen'schen Sprachzeichner graphisch darzustellen*, Zt. f. Biol., 1887 XXIII 314, Tafel III, Fig. 21 B.

[2] HERMANN UND MATTHIAS, *Phonophotographische Mittheilungen, V. Die Curven der Consonanten*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 255, Tafel II.

mains constant shows that the resonance tone or tones change independently. The tongue probably moves while the cords remain at a constant tension. This example of *l* thus differs from those of HERMANN and MATTHIAS.

The labial *b*.

In the spoken words on the gramophone plate the sound *b* follows immediately upon the *ll* without pause. The speech curve at this point (Fig. 71, line 5) shows no measurable vibrations, the enlargement not being great enough to reveal the details of the weak tone of the *b*. The interval occupied is 96^σ.

The vowel *i*.

The vibrations (Fig. 71, lines 6 and 7) have constant period of 2.8^σ. They start with an amplitude of 0 and rises steadily to an amplitude of 0.2^{mm}. At the end they fall to 0 suddenly in four vibrations (Fig. 71, line 8). The pitch of the mouth tone could not be determined. This *i* seems a rather weak vowel when compared with the *i* in *ai*. The sudden ending indicates a quick cut by the following *th* (see above p. 31). The last four vibrations (Fig. 71, line 8) differ somewhat in character from the others and seem to indicate a diphthongal ending to the *i*.

The sonant post-dental *dh*.

As can be heard from the gramophone plate, the *i* sound in *be* is cut short by the *dh* of *the*. This sound appears in the tracing (Fig. 71, line 8) as a space with faint waves following immediately on the sudden fall of the *i* vibrations; the scale of enlargement is not sufficient to give definite information concerning the waves of the *dh*. This sound occupies a time of 56^σ.

The indefinite vowel *a*.

This vowel follows *dh* in *the*. It rises somewhat rapidly to its maximum, remains at an even amplitude (Fig. 71, line 9), and drops suddenly to 0 in the last 4 vibrations. It has a pitch of 6.7^σ on an average and a maximum amplitude of 0.4^{mm}. The entire vowel contains 12 cord vibrations and occupies a total time of 84^σ.

The *ap* glide.

The vowel *a* of *the* is cut short by the closing of the lips for *p*. This suddenly reduces the amplitude of the vibrations till they are very faint (Fig. 71, line 9), yet the cords continue to vibrate after the closure as may be seen from the faint vibrations (Fig. 71, lines 9 and 10). The

sound can no longer be considered to be the vowel *a* and cannot in the usual sense be called a *p*. It may be treated as a glide although it occupies fully two thirds of the interval of 112^{σ} between the *a* in *the* and the *a* in *parson.*

The labial *p*.

If the period of sonancy after *the* is to be considered as a glide, the remaining third of the 112^{σ} may be assigned to the *p* (Fig. 71, line 10).

The vowel *a*.

The word *parson* appears to the ear (E.W.S.) to have an inflectional force of the form indicated in Fig. 72, as often appears at the end of questions; the circumflexion appears to lie in the *a* and the deep fall to be in the *n*; this word seems to contain a trace of an *r*. This word differs

FIG. 72.

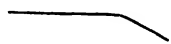


FIG. 73.

from the same word four lines later (p. 15) which appears to the ear to have a deep inflectional tone, at first level and then falling as in deciding a matter; this is indicated in Fig. 73. This latter word seems to contain no *r*. The word *parson* is in both cases apparently continuous with the word *the* and would be acoustically written *theparson.*

The vowel *a* in this case occupies a period of 180^{σ}. It is preceded by the interval of 112^{σ} belonging to the *p* and is followed by a glide of 12.3^{σ}.

It shows 36 cord vibrations. The pitch rises gradually as shown by the following measurements of the successive periods: 6.7, 7.0, 6.7, 6.0, 6.0, 6.3, 5.3, 5.3, 5.3, 5.3, 5.3, 5.3, 4.9, 4.9, 4.6, 4.6, 4.6, 4.6, 4.6, 4.2, 4.2, 4.2, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 3.9, 4.0, 4.2.

It contains a constant lower resonance tone with a period of 2.8^{σ} or a frequency of 357 (Fig. 35).

The upper resonance tone is one of about 714 vibrations per second.

The amplitude rises through the first four vibrations from zero to 0.3^{mm} and is maintained at this to the end.

The vowel *a* in *parson* has undoubtedly a diphthongal character. The first portion resembles the *a* sound discussed above (p. 16) in the rising cord tone but differs radically in the falling resonance tone, in which respect it is somewhat like the *a* in *die* (Figs. 49 and 53). The latter

portion (Fig. 71, line 13) is related to the earlier portion much as the *i* is related to the *a* in *ai* in respect to amplitude, the lowering of the resonance tone and the continuance of the cord tone. Although this latter portion is not so long as in most cases of *ai*, the resemblance is sufficient to justify the statement with which this paragraph begins. The sound might be written *a*× where the sign × indicates a brief vowel not yet determined. It may be suggested that this brief vowel may arise from the weakening of the *r*, whereby a vowel sound partially or completely replaces the full *r*. It seems, however, to be a general rule, that in English long vowels have a diphthongal character.

The *ar* glide.

The sudden fall in amplitude and the change in pitch of the vowel × in *a*× is continued through an interval of 8.8^σ in which 3 vibrations with a period of 2.4^σ appear (Fig. 71, line 13, middle). During this time the tongue is presumably passing to the *r* position.

The liquid *r*.

The very brief *r* is distinctly heard in the word *parson*; it occupies a time of 63^σ (Fig. 71, line 13 middle to line 14 beginning).

The *r* shows clearly 3 "pseudo-beats"[1] with a period of 19^σ or a frequency of 53. The vibrations within the beats are grouped in pairs indicating a cord tone acting upon a resonance cavity. The period of the cord tone is at first constant at 3.5^σ (frequency 286) but falls slightly in the third beat. The resonance tone has a period apparently constant at 1.4^σ (frequency 714). Still higher resonance tones are probably present. The following explanation of this curve is proposed tentatively. The *r* consists of a cord tone with a frequency of 286 acting upon a resonating cavity adjusted to a frequency of 714. The tongue is adjusted to vibrate with a frequency of 53; this vibration of the tongue closes and opens the air passage so that the intensity of the sound escaping from the mouth is regularly varied from zero to a maximum and again to zero at the rate of 53 times a second.

The pseudo-beats with the cord and resonance vibrations are shown in the curves by WENDELER[2] and in those by NICHOLS and MERRITT.[3] The German rolled *r* of WENDELER has a much longer beat period, in general over 250^σ or ¼ sec.; the Finnish *r* of PIPPING has a beat of ⅓

[1] WENDELER, as before, p. 304.

[2] WENDELER, as before, Tafel II.

[3] NICHOLS AND MERRITT, *The photography of manometric flames*, Physical Review 1898 VII 93, Plates I and II.

to ½ sec.[1] The American rolled *r* of NICHOLS and MERRITT has also apparently a long beat-period as far as can be judged from the pictures. The brief *r* in three examples given by these last observers has apparently a shorter beat-period than that of *parson*. The cord period in WENDELER's examples varies apparently from 2.3^σ to 3.3^σ (WENDELER's own computation of a frequency of 200 or a period of 5^σ can hardly be correct); the resonance period lies in the neighborhood of 1.7^σ, according to my calculation from his records.

The sibilant *s*.

This follows directly upon the *r*. The vibrations in the curve are hardly distinguishable and no very definite limit can be set to them.

The liquid *n*.

This follows immediately on *s* (Fig. 71, line 14 to end). It occupies an interval of 197^σ. The successive vibrations occupy periods of 4.2, 3.5, 5.1, 3.7, 5.3, 4.1, 4.1, 5.3, 4.2, 4.9, 4.9, 5.3, 5.3, 5.3, 5.3, 5.3, 5.6, 5.3, 5.3, 5.6, 5.6, 5.3, 6.7, 6.3, 6.7, 6.7, 7.0, 7.0, 7.0, 7.0, 7.0, 8.4, 8.8, 8.8, 9.1, 8.8. The maximum amplitude is 0.1^{mm}.

IV. THE NATURE OF VOWELS.

To the question, "What is a vowel?" several kinds of answers may be given.

A vowel may be defined as the sound produced by a certain action of the vocal organs. Some specially peculiar position of one or more of the organs is usually selected as characteristic. Nearly every writer on phonetics gives a definition whose elements are the positions of the vocal organs. Such a definition may be called a "physiological definition of a vowel."

Another method of defining a vowel consists in giving the physical character of the sound of which it consists. This method was proposed by WILLIS who justifies it by the following considerations:

"The mouth and its apparatus were constructed for other purposes besides the production of vowels, which appear to be merely an incidental use of it, every part of its structure being adapted to further the first great want of the creature, his nourishment. Besides, the vowels are mere affections of sound, which are not at all beyond the reach of human imitation in many ways, and not inseparably connected with the human organs, although they are most perfectly produced by them; just so,

[1] PIPPING, *Zür Phonetik d. finnischen Sprache*, Mém. de la Soc. finno-ougrienne, XIV, Helsingfors 1899.

musical notes are formed in the larynx in the highest possible purity and perfection, and our best musical instruments offer mere humble imitations of them; but who ever dreamed of seeking from the larynx an explanation of the laws by which musical notes are governed? These considerations induced me, upon entering on this investigation, to lay down a different plan of operation; namely, neglecting entirely the organs of speech, to determine, if possible, by experiments upon the usual accoustical instruments, what forms of cavities or other conditions, are essential to the production of these sounds, after which, by comparing these with the various positions of the human organs, it might be possible, not only to deduce the explanation and reason of their various positions, but to separate those parts and motions which are destined for the performance of their other functions, from those which are immediately peculiar to speech (if such exist)."[1]

WILLIS's idea of studying the physical characteristics of a vowel has been developed by a series of later observers, finding its full expression in the study of curves of speech by the investigators referred to in Section I (p. 2). In its perfection the "physical definition of a vowel" will consist of a mathematical expression for the course of the molecular vibration of the air which it involves.

A third method of defining a vowel might be proposed, namely, a summarization of its mental characters as perceived by the person hearing it. This might be called a "psychological definition." It would consist in a statement of the pitch of the vowel as heard, whereby reference might be made to some standard musical instrument in determining the pitch; also in a statement concerning its apparent intensity; also one concerning its apparent length; and finally one concerning its expressive character. Such definitions have not before been given; they have been crudely attempted in some cases of the vowels I have studied in the preceding pages.

WILLIS'S *theory*.

Probably the earliest well-founded statement in regard to the nature of vowels was that of WILLIS. His line of thought was as follows:

"It is agreed on all hands, that the construction of the organs of speech so far resembles a reed organ-pipe, that the sound is generated by a vibratory apparatus in the larynx, answering to the reed, by which the pitch or the number of vibrations in a given time is determined; and that this sound is afterwards modified and altered in its quality, by the

[1] WILLIS, *On vowel sounds, and on reed-organ-pipes*, Trans. Camb. Phil. Soc., 1830 III 231.

cavities of the mouth and nose, which answer to the pipe that organ builders attach to the reed for a similar purpose."

WILLIS fitted a reed to the bottom of a funnel-shaped cavity and obtained sounds resembling vowels by modifying the opening of the cavity. He then tried closed cylindrical tubes of different lengths and found that different vowel-like sounds were produced by different lengths of the tube. His experiments led him to the conclusion that the vowel-like sounds are produced by the repetition of one musical note in such rapid succession as to produce another. "It has been long established, however, that any noise whatever, repeated in such rapid succession at equidistant intervals as to make its individual impulses insensible, will produce a musical note. For instance, let the musical note of the pipe be g'', and that of the reed c', which is 512 beats a second, then their combined effect is $g'' \cdots g'' \cdots g'' \cdots g'' \cdots$ (512 in a second) in such rapid equidistant succession as to produce c', g'' in this case producing the same effect as any other noise, so that we might expect *à priori*, that one idea suggested by this compound sound would be the musical note c'.

"Experiment shows us that the series of effects produced are characterized and distinguished from each other by that quality we call the vowel, and it shows us more, it shows us not only that the pitch of the sound produced is always that of the reed or the primary impulse, but that the vowel produced is always identical for the same value of s [the length of the pipe]. Thus in the example just adduced, g'' is peculiar to the vowel $A°$ [*å* as in all]: when this is repeated 512 times in a second the pitch of the sound is c', and the vowel is $A°$: if by means of another reed applied to the same pipe it were repeated 340 times in a second, the pitch would be f, but the vowel still $A°$. Hence it would appear that the ear in losing consciousness of the pitch of s [the length of the pipe] is yet able to identify it by this vowel quality. But this vowel quality may be detected to a certain degree in simple musical sounds; the high squeaking notes of the organ or violin speak plainly *I*, the deep bass notes *U*, and in running rapidly backwards and forwards through the intermediate notes, we seem to hear the series *U, O, A, E, I, I, E, A, O, U,* etc., so that it would appear as if in simple sounds, that each vowel was inseparable from a peculiar pitch, and that in the compound system of pulses, although its pitch be lost, its vowel quality is strengthened." . . . "Having shown the probability that a given vowel is merely the rapid repetition of its peculiar note, it should follow that if we can produce this rapid repetition in any other way, we may expect to hear vowels. ROBINSON and others had shown that a quill held against a

toothed wheel, would produce a musical note by the rapid equidistant repetition of the snaps of the quill upon the teeth. For the quill I substituted a piece of watch-spring pressed lightly against the teeth of the wheel, so that each snap became the musical note of the spring, The spring being at the same time grasped in a pair of pincers, so as to admit of any alteration in length of the vibrating portion. This system evidently produces a compound sound similar to that of the pipe and the reed, and an alteration in the length of the spring ought therefore to produce the same effect as that of the pipe. In effect the sound produced retains the same pitch as long as the wheel revolves uniformly, but puts on in succession all the vowel qualities, as the effective length of the spring is altered, and that with considerable distinctness, when due allowance is made for the harsh and disagreeable quality of the sound itself."

Thus WILLIS maintains two theses: 1. that a vowel consists of [at least] two tones, a cord tone and a mouth tone; 2. that the mouth tone is independent of the cord tone in regard to pitch.

The first of these theses led to attempts to determine the pitch of the mouth cavity; the results will be considered in Section V below.

The second thesis was for a long time entirely neglected in favor of another one, although, as I hope to show, it is the one that correctly represents the facts.

HELMHOLTZ's *theory*.

According to HELMHOLTZ the vowels arise from the vibrations of the vocal cords through the strengthening of certain overtones by the resonance of the mouth.

"We may well suppose, that in tones of the human larynx, as in those of other reed instruments, the overtones would continuously diminish in intensity with rising pitch, if we could observe them without the resonance of the mouth. In fact they correspond to this assumption fairly well in those vowels that are spoken with widely opened, funnel-like mouth-cavities, as in sharp *A* or *Ä*. This relation is however very materially changed by the resonance in the mouth. The more the mouth-cavity is narrowed by the lips, teeth or tongue, the more prominently its resonance appears for tones of very definite pitch, and by just so much more it thus strengthens those overtones in the tone of the vocal cords which approximate the favored degrees of pitch; and by just so much more the others are weakened."[1]

The pitch of the tones for which the mouth resonates best was studied

[1] HELMHOLTZ, Die Lehre v. d. Tonempfindungen, 4. Aufl., 170, Braunschweig 1877.

by HELMHOLTZ by means of tuning forks held before the mouth. The resonance differed for different vowels.

"The pitch of the strongest resonance of the mouth depends only on the vowel for whose production it has been arranged, and changes essentially even for small changes in the character of the vowel as for example in various dialects of the same language. On the other hand the resonances of the mouth are almost independent of age and sex. I have found in general the same resonances for men, women and children. What is lacking to the childish and female mouth in capacity can be easily replaced by narrower closure of the opening, so that the resonance can still be as deep as in the larger male mouth."

According to HELMHOLTZ "the vowel sounds are different from the sounds of most musical instruments essentially in the fact that the strength of their overtones depends not only on the number of the overtone but above all on its actual pitch. For example, when I sing the vowel *a* or the note E#, the reinforced tone is b_2, or the 12th one, and when I sing the same vowel on the note b_1 it is the second one."[1]

This view of HELMHOLTZ necessitates the assumption of an accommodation of the resonance tone to the voice tone within quite a range; thus as the voice tone rises or falls the mouth must also change its tone or be able to extend its resonance to a considerable degree. This assumption was made by HELMHOLTZ, the range of accommodation being supposed to extend over as much as an interval of a fifth in music each way from the tone of best resonance. This view has been called the "accommodation theory." According to this theory the mouth must accommodate itself to one overtone of the voice tone and when this rises or falls to a considerable degree it must readjust itself to some other one in order to keep the resonance tone within a limited range.

The difference between the theories of WILLIS and HELMHOLTZ lies chiefly in the relation between the mouth tone and the voice tone; for the former there was no relation, for the latter the resonance tone was one of the overtones of the cord tone.

"WILLIS's description of the acoustic movement in the vowels doubtless coincides closely with the truth; but it gives only the manner in which the motion occurs in the air, and not the corresponding reaction of the ear to this motion. That even such a motion is analyzed by the ear according to the laws of resonance into a series of overtones is shown by the agreement in the analysis of the vocal sound when it is executed and by the resonators."[2]

---

[1] HELMHOLTZ, as before, 191.

[2] HELMHOLTZ, as before, 191.

HELMHOLTZ also devised an apparatus of electric tuning forks and produced vowel-*like* sounds by combining a fundamental tone with different sets of overtones.

HELMHOLTZ was greatly influenced in his theory by his views of the action of the ear. The hypothesis that all regular vibratory movements reaching the ear are analyzed by it into a series of harmonics of the fundamental period is an assumption that seems to lead necessarily to the HELMHOLTZ theory. This assumption, however, we must disregard at the present time; the problem concerns the nature of the vibratory movement characterizing a vowel and the solution must be found in an unbiased analysis of the vowel curve; the question of how the ear acts is a later one.

PIPPING'S[1] work with HENSEN'S instrument (see above, p. 4) led him to the following conclusions.

"In agreement with HELMHOLTZ I have found that each vowel is distinguished by one or more regions of reinforcement of constant pitch. The intensity of its partial tone is, *cæteris paribus*, greater as it coincides more accurately with the range of reinforcement.

"In regard to the range of the reinforcement I cannot agree with HELMHOLTZ. HELMHOLTZ indeed states that the range can be different according to the opening of the mouth, the firmness of walls of the oral cavity, etc. But he lays so little weight on this difference that he does not attempt to use it in the characterization of the different vowels. To judge from page 183 of the Lehre von den Tonempfindungen HELMHOLTZ thinks that the range of reinforcement must extend in general at least a musical fifth above and below, and this is certainly not the case.

"Sung vowels contain only harmonic partial tones." That is, a vowel produced by singing consists of a series of tones whose vibrations stand in the relations of 1 : 2 : 3 : 4 : ···.

"The intensities of the various partial tones do not depend to any essential degree on their ordinal numbers." That is, in distinction to most musical instruments it is not the fact that the first partial is much the stronger and that the higher partials are in general weaker.

"The various vowels differ from each other in ranges of reinforcement which are of different numbers, width and position in the scale of pitch." That is, one vowel may have two ranges of reinforcement, another three, etc., and these ranges may differ.

On a later occasion[2] PIPPING believes that the range of accommodation may exceed even the limits allowed by HELMHOLTZ.

---

[1] PIPPING, *Zur Klangfarbe der gesungenen Vocale*, Zt. f. Biologie, 1890 XXVII 77.
[2] PIPPING, *Zur Lehre von den Vokalklängen*, Zt. f. Biologie, 1895 XXXI 573, 583.

Comparison of the two theories.

The two conflicting theories require a decision concerning their validity.

Among the results that support the view of WILLIS we may notice those obtained by DONDERS with the SCOTT phonautograph.[1]

"Each of the fourteen vowels when sung on a constant tone produces a constant curve. . . . "For each vowel the form of the curve changes with the pitch. This result is connected with the peculiarity of the vowels, that their timbre is determined not by overtones of a certain order to the fundamental, but rather by overtones of a nearly constant pitch."

This last statement implies the fact that if the resonance tones of the mouth were overtones of the voice tone bearing a definite relation to it, such as 1st, 2d, the curve would remain the same in form no matter what the pitch, just as the curve of vibration for a violin string has a typical form which persists in spite of changes in the pitch of the string. On the other hand if the tone of the mouth is a constant one, as WILLIS assumes, the combined vibration produced by the voice tone and the mouth tone would change for any change in pitch of the voice tone.

HERMANN'S investigations were carried out by transcribing the curves of song from the phonograph.[1] He finds that the essential fact in a vowel is the intermittent or oscillatory blowing of the mouth tone by the voice. Under such circumstances it makes no difference whether the resonance tone coincides with any fraction of the voice tone period or not.[2] HERMANN thus supports the theory of WILLIS in asserting that the mouth tone is completely independent of the voice tone. To this statement HERMANN adds that of the intermittence of the voice tone which seems never to have been suspected by previous observers. This new fact of intermittence appears much more clearly in my curves of the spoken *a* (see Figs. 7, 17, 30) than it does in HERMANN'S curves of the sung vowels. HERMANN believes that this intermittence is essential to the production of a vowel and that merely adding a constant tone to a complex of tones does not give a vowel.[3] This intermittence, however, occurs only in some vowels of low pitch, as in the first portions of the cases of *a* just mentioned; it does not occur in the *i*. Even in the latter portion of my

[1] DONDERS, *Zur Klangfarbe der Vocale*, Annalen de Physik u. Chemie, 1864 CXXIII 528.

[2] HERMANN, *Phonophotographische Untersuchungen*, Archiv f. d. ges. Physiol. (Pflüger), 1890 LXXIV 380, 381.

[3] HERMANN, *Weitere Untersuchungen ü. d. Wesen der Vocale*, Archiv f. d. ges. Physiol. (Pflüger), 1895 LXI 192.

cases of *a* it is hardly proper to speak of intermittence; the pressure in the wave from the voice tone is not evenly distributed throughout the period, but there is nothing resembling intermittence. Even in HERMANN's own curves for *i* as shown, for example, in one of his latest publications,[1] there is no such intermittence.

According to HERMANN each vowel has one or two fixed mouth tones whose pitch varies within narrow limits if at all; these tones he calls "Formants." Thus, the vowel *u* when sung by a certain person contains not only the voice tone but also one or two mouth tones; these mouth tones are the same when the same vowel is sung at different pitches.

HERMANN has objected to the overtone theory of the mouth tone that in many voices the formant is so high above the voice tone that it cannot be supposed that an overtone of that pitch could possibly be present. Thus as the voice-tone *G* the vowel *i* has a strong mouth tone that would correspond to the 28th or 29th partial of the voice tone, whereas such a high partial, if present at all, would be too weak to be heard.[2]

A final decision in the case of the vowel *a* can, I believe, be established on the basis of the curves described above in Section I. The independent tone theory is certainly the only one that will account for this vowel. In the first place the vowels studied were spoken vowels and were open to none of the objections that may be made against sung vowels. In the second place the resonance vibrations can be seen starting at regular intervals and dying away completely in some instances and less completely in others within a single period of a voice tone. Again, the resonance vibration can be seen to remain of constant period while the voice tone rises through a distance of several octaves *within one single vowel.*

In the face of such conclusive evidence it is hard to see any point in which the decision in favor of the theory proposed by WILLIS and developed by HERMANN can possibly be attacked. It is natural to assume that a theory found to be valid for one vowel will be valid for all; it is, of course, possible that other laws may hold good in other vowels, but until this possibility is proven we can treat all vowels on the independent-tone theory.

The noise theory.

Another view of the way in which the resonance tone is aroused resembles an older view of the action of organ pipes. "The concomitant resonances [mouth tones] which create or constitute vowel quality are

[1] HERMANN, *Weitere Untersuchungen über d. Wesen der Vocale*, Archiv f. d. ges. Physiol. (Pflüger), 1895 LXI Tafel V.

[2] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 274.

animated, primarily and essentially, by the irregular noises which issue, *together* with the vocal tone from a speaking or singing glottis, but *without* it from a whispering one. Some of these are always found capable of affording just the appropriate impulse, and of kindling the resonances of the configuration [mouth cavity]."[1] This view is undoubtedly correct as far as whispered vowels are concerned, but it can hardly be supported for spoken vowels. In one respect the case is analogous to that of an ordinary resonator; by blowing against the opening or by tapping the walls the tone of the resonator can be faintly heard. Thus, in whispering, the vowels can be produced with faint tones. These faint tones are, however, quite different affairs from the strong mouth tones of spoken vowels although they may be of the same pitch. In speaking there must be a stronger force to set the mouth cavity in vibration than the faint noises that accompany the cord tone; otherwise the mouth tone would be quite overpowered by the cord tone and there would be no noticeable difference between vowels spoken on the same note. Moreover, noises seem to have no power to arouse strong resonances; thus the noise of *s*, though loud and produced directly on the edge of the resonance cavity, does not produce any marked resonance vibrations (p. 70). The force that sets the mouth cavity in vibration can only come from the cord tone and the "noise theory" of vowels may be definitely laid aside.

Observations on the nature of spoken vowels.

Previous investigators have had in mind almost exclusively the vowels sung on musical notes. It has been universally assumed that the spoken vowels do not differ essentially from the sung ones. Thus HERMANN says, "The difference between sung and spoken articulation lies exclusively in the fact that the pitch, intensity and duration of the syllables—or more accurately, of the vowels—are governed in song by melody and rhythm and in speech by the laws of emphasis according to meaning and arrangement. In a single vowel there can thus be absolutely no difference between song and speech."[2]

My investigations show, I believe, that this view is erroneous.

In the first place the voice tones of spoken vowels are seldom of constant pitch. Some are nearly constant in pitch, some fluctuate, some rise and fall in various simple or complicated ways. I have looked over hundreds of vowels in the records and find that there is a typical tone for the whole discourse which occurs in a majority of the vowels, while the others

[1] LLOYD, *Speech sounds: their nature and causation*, Phonet Stud., 1890 III 277.

[2] HERMANN UND MATTHIAS, *Phonophotographische Untersuchungen*, Archiv f. d. ges. Physiol. (Pflüger), 1894 LVIII 258.

have quite different tones. Many of the vowels are fairly constant, but many others vary. Indeed, it is just such changes and fluctuations in pitch and also in intensity that enable the voice to express the character of the thought. Without these changes the speech would be a monotonous sing-song resembling the speech of the deaf who have been taught by the oral method. When words are sung, they lose most of their character; speech is capable of expressing by its modulations the various emotions and conditions of the individual, whereas the singer has few resources at his command.

In the second place vowels have certain characteristic laws of pitch and intensity in certain positions. Thus the *a* of *ai* in my curves begins practically at zero in both pitch and intensity. The *i* has a nearly constant pitch with a slight fall, and a peculiar rise and fall of intensity. Presumably we shall at some time be able to determine the analytical expressions for the vowels and shall find that their properties follow definite laws.

It is interesting to note that this change in pitch in the spoken vowels has so generally escaped notice. I know of only one recorded observation that might refer to the subject.

ARISTOXENUS,[1] in discussing *κίνησις φωνῆς* opposes *κίνησις συνεχής* to *κίνησις διαστηματική*. The first term may be translated as "change in pitch of the voice," the second as "continuous change," and the last as "change by steps." The continuous change he considers to be characteristic of speech as opposed to song. "Now the continuous movement is, we assert, the movement of conversational speech, for when we converse the voice moves through a space in such a manner as to seem to rest nowhere."[2] It is not quite clear to me what he means by "continuous change." If he had definitely in mind the change in pitch of a vowel within itself, he certainly furnishes an example of most precise hearing and careful observation whereby he anticipates a result arrived at later only by careful experimental methods. I am somewhat inclined to doubt that he had in mind anything more than the general observation that in speech the voice rises and falls irregularly, yet the special statement that the changes are *continuous* necessarily involves the changes within single vowels.

One of the most curious facts observed in the vowels studied in the previous section is the change of the resonance tone. The pitch of the

---

[1] ARISTOXENUS, Harmonica, I § 25, p. 8, Meib. The passages are collected in JOHNSON, *Musical pitch and the measurement of intervals*, Thesis, Baltimore 1896.

[2] ARISTOXENUS, Harmonica, I § 28, p. 8, Meib., quoted in JOHNSON, *The motion of the voice in the theory of ancient music*, Trans. Amer. Philol. Assoc., 1899 XXX 47.

resonance tone is frequently not a fixed one but one altered according to some law. In most of the cases of the *a* it begins to change in the latter portion; in the *i* it is frequently constant but often falling.

To the foregoing account of vowels it is necessary to make some additions. The most important one is the statement that a vowel is not a fixed thing, but a changing phenomenon. There is no such thing as a vowel *a* with a definite character under all circumstances. Even for the same speaker there are continual changes and variations in this vowel. For different speakers, for different dialects and for different languages the changes become so great that the *a* finally has little resemblance to the one chosen as a standard. We may say that a large number of our speech sounds may be classed together by a more or less close resemblance and may be designated by the term *a*. A similar statement would hold good of any speech sound.

The changes from *a* take place in all directions, in voice tone, in mouth tone, in length, etc. By selecting examples properly a continuous series can be made of forms whose members differing but little from their neighbors, reaching from *a* to any of the other vowels. For example, between a typical *a* and a typical *o* all the intermediate vowels may be found corresponding to the position of the mouth between the *a* position and the *o* position. "In no language or dialect are the sounds which pass current for one and the same vowel absolutely identical. They vary perceptibly in individual use: and hence . . . a vowel is not one single definite sound, but a group of more or less closely resembling sounds which in a given speaking commuity pass current as one vowel. There seems to be no practical limit to the range of this wandering so long as the sounds employed do not actually *overlap* those of any other vowel which happens to be used in the same language."[1]

Mechanical action in producing vowels.

Although it may be regarded as settled that a vowel consists of a cord tone with its overtones and one or more resonance tones from the mouth and possibly from the pharynx,[2] there still remains the physical problem of the method in which the cord tone arouses the resonance tone.

The mouth cavity with the pharynx and vocal cords may be considered as a pipe with membranous reeds. The theory of its action will be similar to that of an ordinary reed organ pipe.

Each vibration of the reed sent a wave of condensation and rarefaction along the pipe. When the pipe is of such a length that this wave is re-

---

[1] LLOYD, *Speech sounds: Their nature and causation*, Phonet. Stud., 1890 III 254.

[2] LLOYD, brief note in Proc. Brit. Assoc. Adv. Sci., 1891 796.

flected back in such a way as to reinforce the vibration of the reed, the resonance tone is a loud one. Thus, when a properly adjusted resonator is placed behind a vibrating fork the tone of the fork is strongly reinforced. The reinforcement is also strong when the resonator coincides in pitch with an overtone of the reed.

Such a coincidence between the periods of the pipe tone and the reed tone is not necessary. Each impulse from the reed may be considered as striking the pipe with something of the nature of a blow, whereby the proper tone of the pipe itself may be aroused for an instant. The pipe may thus have its own pitch and be heard, no matter what relation there may be between it and the pitch of the reed. When the blow from the reed is rapidly repeated, both the reed tone and the pipe tone will be heard.

Such a method of producing resonance tones has been declared to be impossible hy HENSEN,[1] who remarks that air from a reed pipe cannot arouse a resonance tone. The experiment on which he bases this statement consisted in placing a resonator at the end of a reed pipe. At a certain pressure of air the pipe sounded its own tone, at a different pressure it was silent. The resonator sounded only when the pipe was silent. Nevertheless there were occasions when both the pipe tone and the resonance tone appeared together; these were called by HENSEN unsuccessful experiments. We ought perhaps to call them rather the successful ones.

To these experiments and deductions HERMANN replied that a labial pipe can be used to sound a reed pipe, and some experiments were made to demonstrate the fact.[2] I have attempted in another way to show that a series of puffs of air of any periodicity may be used to sound a labial pipe of any pitch.

A disc with its edge cut into waves forming approximately a sine-curve was rotated by an electric motor at any desired speed. Its edges passed between the ends of two pieces of rubber tubing so arranged that the air blown into one of them passed directly into the other one if the waves of the disc permitted; the position was so chosen that the waves of the disc regularly interrupted the air current completely. The end of the rubber tubing was flattened and placed so as to blow against the edge of a piece of brass pipe stopped at the other end. The experiment began with the disc at rest. A current of air was blown through the tubing; the pipe gave forth a tone. The disc was then set in rotation; the tone

---

[1] HENSEN, *Die Harmonie in den Vocalen*, Zt. f. Biol., 1891 XXVIII 39.

[2] HERMANN, *Weitere Untersuchungen ü. d. Wesen d. Vocale*, Arch. f. d. ges. Physiol. (Pflüger), 1895 LXI 195.

of the pipe was regularly intermitted. As the disc moved faster, this intermittence became more rapid. Finally, the intermittence itself was heard as a tone in addition to the pipe tone. Thus an intermittent air current, such as is employed for producing tones directly, can be used to produce a pipe tone in addition.

I have even succeeded in arousing the resonance of a closed tube by blowing through an artificial larynx. The artificial larynx was made by binding a piece of thin soft rubber around the end of a glass tube. Two opposite points of the thin-walled rubber tube thus made were each caught between the thumb and finger; the tube was then stretched till the sides come together. A blast of air through the tube set these edges in vibration and produced a tone. By placing the edges at the right spot over the mouth of a bottle or a test-tube or a key (Fig. 74) the resonance tone of the latter could be distinctly heard.

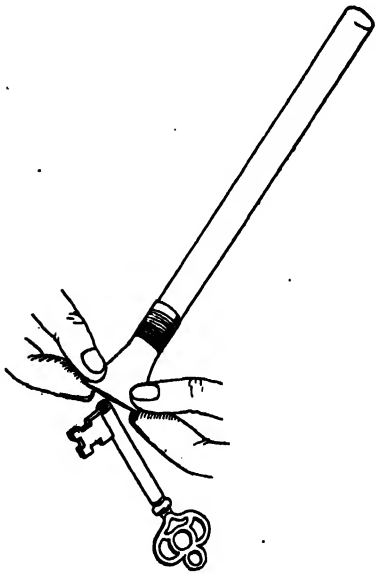


FIG. 74.

When the edges of the artificial larynx are properly placed against the opening of a small tube such as the hole of a door-key, the tone of the key is heard loudly in addition to that of the artificial larynx. The pitch of the larynx tone may be altered at will. This experiment illustrates with great vividness the method in which vowels are actually produced in the vocal organs.

It is not so easy to arouse a tube of low pitch such as a bottle in this way, because the volume of air passing through the artificial larynx is not large.

It can thus be regarded as definitely settled that the current of air from a reed can be used to arouse a resonance tone in a cavity properly adjusted to receive the air. The WILLIS theory of vowel production is therefore at least a physical possibility.

To this statement we may add that the reed tone and the resonance tone may vary independently of each other, but that the resonance tone is loudest when its pitch is higher than that of the reed tone.

WILLIS's view of the way in which the resonance tone was superimposed on the reed tone is very explicit. "According to EULER, if a single

pulsation be excited at the bottom of a tube closed at one end, it will travel to the mouth of this tube with the velocity of sound. Here an echo of the pulsation will be formed which will run back again, be reflected from the bottom of the tube, and again present itself at the mouth where a new echo will be produced, and so on in succession till the motion is destroyed by friction and imperfect reflection. . . . The effect therefore will be a propagation from the mouth of the tube of a succession of equidistant pulsations alternately condensed and rarefied, at intervals corresponding to the time required for the pulse to travel down the tube and back again; that is to say, a short burst of the musical note corresponding to a stopped pipe of the length in question, will be produced."[1]

The true view of the action of the mouth in producing a resonance tone seems to be the following one. The sudden puff of air from an explosive opening of the cords may be considered to act as a piston compressing the air before it in the mouth cavity. The air acts as a spring by its resistence to compression and drives the piston back beyond its position of equilibrium; the resistance to dilatation draws it back, and so a vibratory movement is set up. Under these circumstances the air acts merely as a spring; the form of the cavity is immaterial and the period of vibration remains the same, provided the capacity be not varied. The single impulse of the piston thus makes the resonator a source of vibration, whose period remains practically constant but whose amplitude steadily diminishes from loss of energy mainly by communication to the external air. Such vibrations are seen in the curves for *a* in Section II above. This statement is an adaptation of that given by Rayleigh for resonators in general.

The question arises as to the period of the tone thus produced by the resonator.

There are cases in which the Helmholtz view of the action of the mouth cavity might seem to have a possibility of correctness. If we assume (1) that a uniform condition has been attained, (2) that the natural period of the resonator does not differ greatly from that of the cord period, and (3) that the cord vibrations are of not too explosive a nature, it follows that the effect of the resonator can only be to modify the intensity and phase of the partials of the cord note. The partial or partials nearest to the natural periods of the mouth cavity will be reinforced and they can be found from the cord by the Fourier analysis.

Under the assumptions made above the vibration of the resonance cavity is a *forced* one, and the conclusion concerning the section of the

[1] Willis, as before, 243.

mouth cavity is necessarily correct.[1] The first and second assumptions made above have been explicitly stated by RAYLEIGH, who concludes that both the WILLIS and the HELMHOLTZ ways of treating the action of the mouth cavity are legitimate and not inconsistent. "When the relative pitch of the mouth tone is low, so that, for example, the partial of the larynx note most reinforced is the second or the third, the analysis by FOURIER'S series is the proper treatment. But when the pitch of the mouth tone is high, and each succession of vibrations occupies only a small fraction of the complete period, we may agree with HERMANN that the resolution by FOURIER'S series is unnatural, and that we may do better to concentrate our attention upon the actual form of the curve by which the complete vibration is expressed."[2] The two forms of treatment imply that the resonance tone is to be considered in the one case as a free vibration of the air in the cavity, and in the other case as a forced vibration. Some cases of the *i* (Figs. 44 and 53) may be reconciled with the HELMHOLTZ view, the resonance tone being an overtone of the cord tone and changing with it. The cases of *a* and most of those of *i* are decidedly inconsistent with the overtone theory. Possibly the variation from the overtone theory arises from the explosive manner in which the cords open. The general description of their action for *a* probably holds good even when the resonance tone is only about an octave above the cord tone; each puff of air is stronger at the start and fades away, setting the air in the resonance chamber into free instead of forced vibration. This general characteristic can be traced in each *a* even to the point where the resonance tone is slightly less than the octave of the cord tone, as in Fig. 11. We are probably justified in concluding that the WILLIS theory of the production of vowels holds good universally.

V. THE MOUTH TONE IN VOWELS.

DONDERS sought to determine these tones by noting the pitch of the mouth cavity when the various vowels were whispered.[3]

HELMHOLTZ[4] and AUERBACH[5] by holding tuning forks before the mouth when it had been fixed for a certain vowel have found those whose tones are most strongly reinforced. The mouth acts as a resonator and the tone most strongly reinforced is that to which the mouth is tuned. The

[1] RAYLEIGH, Theory of Sound, § 48, 66, 322k, 397, London; 1894, 1896.

[2] RAYLEIGH, as before, § 397.

[3] DONDERS, *Ueber d. Natur. d. Vokale*, Archiv f. d. holländ. Beiträge z. Natur. u. Heilkunde, 1858 I 157.

[4] HELMHOLTZ, Lehre v. d. Tonempfindungen, 4. Aufl., 171, Braunschweig 1877.

[5] AUERBACH, *Untersuchungen ü. d. Natur. des Vokalklanges*, Diss., Berlin 1876.

objections arise: that there is no certainty that the mouth is really in the vowel position desired; and that the mouth may resonate to several tones. The adjustment of the mouth may be quite different when no expiration is occurring from what it is during whispering or speaking or singing.[1] At any rate we have no assurance that it is the same. I quite agree with HERMANN that the only trustworthy determinations of the mouth tone are those obtained by actual whispering, singing or speaking. Whispered vowels were examined by DONDERS, HELMHOLTZ and HERMANN.

The pitch of the mouth tone has been studied in a different way by LLOYD. The mouth, as an excentric cavity, would naturally have two resonance tones: the tone of the "porch" or narrow front part, and the tone of the "chamber" or rear part.[2] A combination of a tube and a cylinder can be made to give a vowel-like sound when the sizes are properly selected. LLOYD produced various vowel-like sounds and determined the tones of the tube and the cylinder. The vowel-character of a sound is, according to LLOYD, essentially determined by the relations of pitch between these two tones, or among several tones when there are more than two.

LLOYD[3] has also mapped out the forms of the mouth cavity involved in different vowels and has calculated the tones to which they would resonate. Thus for the vowels in the following words he has calculated the resonance tones as indicated: *piece* 2816, *pit* 2500, *rein* 2112, *there* 1508, *man* 1431, *half* 1082, *law* 834, *note* 623–444, *put* 528, *blue* 314.

Another method used in seeking the mouth-tone consists in analyzing the curve of vibration representing the vowel into a series of curves representing simple tones and determining which of these tones above the voice tone is apparently the loudest.

A simple tone is defined as one for which the deviation of the material particle from its position of rest is given by an expression of the form

$$y = a \sin \frac{2\pi t}{T}$$

where y is the deviation at the moment t, a the amplitude or maximum value of y, and T the time of one complete vibration of the particle through its positive and negative phases. A curve of this kind is called a "sinu-

[1] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 374.

[2] LLOYD, *Speech sounds; their nature and causation*, Phonetische Studien, 1890 III 275, 278; 1890 IV 39; 1891 V 125.

[3] LLOYD, Proc. Roy. Soc. Edin., March 1898.

soid" or a "harmonic" and such a vibration is said to be sinusoidal or harmonic. The exact expression for such a vibration must give the phase from which the values of t are measured; this is done in

$$y = a \sin \left(\frac{2\pi t}{T} - \varepsilon \right)$$

where ε indicates the time between $t = 0$ and the next preceding moment when $y = 0$.

A number r of sinusoids superimposed give a vibratory movement in which

$$y = \Sigma_{n=1}^{n=r} A_n \sin \left(\frac{2\pi n t}{T} - \varepsilon_n \right) .$$

It can be proven that any single-valued finite periodic function with the period T can be expressed by a series of sinusoids whose periods are T, $T/2$, $T/3$ ···. This is generally known as FOURIER'S theorem.[1] The analysis of such a function into a series of sinusoids is known as the FOURIER analysis.

Likewise a number of sinusoids may be added to produce a vibration resembling some given curve. Such a synthesis can be performed by machines constructed for the purpose, for example, the machine of PREECE and STROH[2] or that of MICHELSON.[3] The curves produced by PREECE and STROH somewhat *resemble* the curves of vowels, but so distantly that they indicate the impropriety of considering a vowel curve as a sum of a series of harmonics.

A vowel curve gives by the FOURIER analysis a series of sinusoids of various amplitudes.[4] Those of greatest amplitude are assumed to be the most prominent tones in the complex tone of the vowel. It is also assumed that the one or more stronger tones after the fundamental are the tones of the mouth.

As an objection to this method we are entitled to say, that the FOURIER analysis is in this case a means of representing a vibratory movement by a formula. We may add that it is nothing more than an interpolation formula by which the value of y can be found for any desired instant of

[1] FOURIER, Theorie analytique de la chaleur, Ch. III, Paris 1822.

[2] PREECE AND STROH, *Studies in acoustics. I. On the synthetic examination of vowel sounds*, Proc. Roy. Soc. London, 1879 XXVIII 358.

[3] MICHELSON, *A new harmonic analyzer*, Amer. Jour. Sci., 1898 (4) V 1.

[4] The scheme for the computation and various essential practical devices are given by HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 47.

time. It is merely one case of a more general method[1] of interpolation by a periodic series; it is thus considered in works on the adjustment of measurements.[2]

Such an interpolation formula remains simply a mathematical tool unless it is found to express the actual nature of the phenomenon measured. It has been assumed by practically all writers, that all musical sounds are really combinations of a series of sinusoidal partial tones: for example, it can be readily demonstrated that a violin string vibrates not only as a whole, but also in halves, thirds, quarters, etc. It is also presumably true that each of these parts produces a sinusoidal vibration of the air. Thus, the peculiar tone of the violin is presumably really the sum of a series of approximately sinusoidal tones. The FOURIER analysis in such a case undoubtedly expresses the nature of the tone.

In the case of sung vowels the assumption that the vocal cords vibrate like reeds, and the further assumption that the mouth acts as a resonator reinforcing one or more of the partial tones of the cord would justify the use of the FOURIER analysis for finding the partial tones of the voice-tone and also the tones reinforced by the mouth, provided these assumptions were proved to be correct.

The vocal cords are certainly to be treated as membranous reeds. In the main their vibrations can be supposed to follow the usual laws.

The other assumption, that the mouth acts also as a resonator to reinforce some of the partial tones of the cord vibration, is certainly not justified (p. 73). The main effect of the mouth is to impose a vibration of its own upon the vibration coming from the cord. The reinforcement of partial tones may possibly be present, but it is certainly not prominent. The FOURIER analysis would be applicable only if the mouth tone were coincident with one of the partial tones of the voice tone; this is, at least generally, not the case in song, as has been indicated by WILLIS, DONDERS and HERMANN, and is certainly not the case in speech as is proven by my curves for *a*. With a mouth tone not coincident with a partial tone the FOURIER analysis may, in a vowel of constant pitch, indicate a reinforcement of the nearest partial vibration, or it may show reinforcement of the two nearest partials above or below. The analysis can thus be used to indicate the approximate pitch of the mouth tone in such a case, although it may not coincide with a partial of the voice tone.[3]

[1] GAUSS, *Theoria interpolationis methodo nova tractata*, Werke III 265, 1876.

[2] WEINSTEIN, Physikalische Maassbestimmungen, I 486, Berlin 1886.

[3] HERMANN, *Phonophotographische Untersuchungen*. Archiv f. d. ges. Physiol. (Pflüger), 1894 LVIII 276.

With vowels of changing pitch, as in my examples of *a*, any attempt to apply the FOURIER analysis would be an absurdity. In this vowel the pitch of the voice tone changes from vibration to vibration. The analysis would be thus utterly different for each vibration and would indicate a different mouth tone every time, whereas the reonance vibrations can be seen in the curves to remain constant.

It is an imaginable hypothesis that, since the period of the voice tone in a rising or a falling vowel is not the constant T but some value $f(t)$ which steadily changes, we might make an analysis into a series of sinusoids whose periods change likewise. We would thus have

$$y = \sum_{n=1}^{n=\infty} A_n \sin\left(\frac{2\pi nt}{f(t)} - \epsilon_n\right)$$

The expression for $f(t)$ would differ for different vowels. Such an analysis might accurately represent the case when a musical sound composed of a fundamental with overtones is reproduced on a phonograph whose speed is constantly accelerated. It might also be applicable to the analysis of a glide produced on a musical instrument like a violin. The curve, however, would be of the same form in each period, which—as DONDERS first pointed out and I have abundantly shown—is not the case in the vowels.

Other methods of finding the pitch of the mouth tone may be used. The method that suggests itself at once is simply that of measuring the length of a wave of the mouth tone. This could best be done in my curves by measuring the length of a set of waves and dividing by the number; though the measurement could not be made to a finer unit than 0.1^{mm} this reduces the error for a set of 5 waves to $\frac{1}{5}$ of 0.1^{mm}, or 0.02^{mm}. This method is applicable only when the vowel curve shows regular vibrations within a single period of the voice tone. When the curve shows irregular or complicated vibrations, some other method would be used.

HERMANN has used three other methods: (1) the centroid method, (2) the method of proportional measurement, and (3) the counting of the vibrations when they exactly fill one period of the voice tone.[1] The last method amounts to the same thing as mine for a particular case. The proportional method is also practically the same for other cases. The centroid method seems to give only approximate results.[2] The term "centroid" seems to me preferable to "center of gravity" used by HERMANN.

[1] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1890 XLVII 359.

[2] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1893 LIII 51; 1894 LVIII 276.

Of all the methods and investigations employed for determining the mouth tone those of HERMANN[1] are entitled to by far the weightiest consideration. He finds for *u* (*oo*) two tones, one in the first part of the first octave and one in the second octave, for *o* (*au*), and *a* a tone in the second octave which rises in pitch as *o* changes to *a*, for *ä* and *ē* a tone in the second octave and one in the third octave, for *ö*, *ü* and *i* a very high tone which is in the middle of the third octave for *ö*, at the end of that octave for *ü* and in the fourth octave for *i*. The octaves are numbered in the German fashion, middle *c* being in the first octave. The resonance tones for my examples of *a* and *i* are given on pages 55 and 56, and those of some other vowels in Section III.

These data give only the approximate regions in which we may expect to find the mouth tone. It is unquestionably true that within these regions the mouth tone will vary for different dialects and different conditions of speech.

The mouth tone need not be a fixed one though it is generally so. A rise and fall of the mouth tone might readily be used as a factor of expression in speech. Several examples of such changes have been given in Section II.

It seems fairly well established that in addition to the cord tone there may be several resonance tones from the mouth cavity. LLOYD distinguishes at least two: that of the front part of the mouth (the porch resonance) and that of the whole mouth (the fundamental resonance).[2] There may be also a resonance tone from the pharynx.[3] The various vowels arise from different "radical ratios" between the porch tone and the fundamental mouth tone,[4] while it is possible to change the pitch of both to some extent. Various other tones may arise from the configuration of the mouth and the coexistence of the tones already mentioned.[5]

Although LLOYD's supposition of the possible presence of a number of resonances in the mouth cavity[6] may be partly justified, yet one of these resonances must far exceed all others in prominence in order to produce the constancy in form and period of the resonance vibrations seen in the

---

[1] HERMANN, *Phonophotographische Untersuchungen*, Arch. f. d. ges. Physiol. (Pflüger), 1894 LVIII 270.

[2] LLOYD, *Speech sounds; their nature and causation*, Phonet. Stud., 1890 III 261.

[3] LLOYD, *Speech sounds; their nature and causation*, Phonet. Studien, 1891 IV 294; also a note in Proc. Brit. Assoc., 1891 p. 796.

[4] LLOYD, *Speech sounds; their nature and causation*, Phonet. Stud., 1891 IV 52.

[5] Same, 207.

[6] LLOYD, *Speech sounds; their nature and causation*, Phonet. Stud., 1890 III 261; 1891 IV 52, 206.

curves examined in Section II. It is doubtful if there are more than two resonances of the mouth that are of any noticeable strength; as explained above (p. 83) the air in a resonance cavity acts as a spring whose period depends on the size while the form of the cavity is immaterial for the chief resonance tone. We must add that, although the additional resonance tones and the overtones of the cord tone may not appear in any record, they undoubtedly give characteristic colors to the final result.

The importance of the pharyngeal resonance has been strongly emphasized by MARICHELLE.[1]

This author maintains the following theses: *A.* The capacity of the buccal resonator does not exercise a characteristic influence on the pitch of the vowels. The statement that the mouth cavity in front of the elevation of the tongue has no influence is based on an experiment in filling the cavity of the palate with wax and finding that the vowels *O* and *OU* can still be pronounced. Compensation for the size of the resonating cavity by change in the lip opening is avoided by forming the opening in a card placed before the mouth. These experiments seem to me too inaccurate and so contrary to our knowledge of the action of resonating cavities that we cannot accept them. Moreover, a vowel like *O* is—to my ear at least—distinctly modified in expression by any change of the mouth cavity although it still remains an *O* until the change is a great one. This can be conveniently tested by inserting two fingers in the mouth; the *O* changes in expression and can be readily made into an *OU* by the proper manipulation. *B.* The dimension of the lip opening constitutes only a general vague and unstable indication of the vowel. *C.* The separation of the jaws does not sufficiently characterize the vocal sounds. *D.* The displacement of the tongue forward or backward furnishes no precise and essential information on the character [timbre] of the vowels. It is possible to produce all the vowels with practically any position of the tongue. "Here again the physiological description, as comprehended generally, gives only accessory facts and no characteristic ones." These three statements are true in a vague way but they do not prove that the vowel character is independent of these factors; the vowels undoubtedly depend essentially and directly on them. MARICHELLE's point, however, seems to be that the essential factor is the size of the resonance cavity and not its exact form; and in this he is presumably correct.

According to MARICHELLE three distinct regions of the mouth are used in forming vowels: 1. the anterior tongue-palate cavity; 2. the pos-

[1] MARICHELLE, La parole d'après le tracé du phonographe, 27, Paris 1897.

terior tongue-plate cavity; 3. the lip opening. The characteristic tones are modified by *a.* the nature of the walls, whether soft or hard; *b.* the capacity of the posterior resonator; *c.* the degree of opening of the tongue-palate orifice; *d.* the lip opening.

MARICHELLE seems to be quite correct in insisting on the importance of the posterior cavity; it is the one into which the vibrations of the cords pass immediately and it undoubtedly acts as a strong resonator. It would be somewhat rash, however, to say that the most prominent resonance vibration comes from this cavity. It may be suggested that the vowel is a complex of resonance tones of which the pharyngeal tone would be one, the anterior mouth tone another, and so on.

The assumption of PIPPING[1] that the chief resonance tone of the vowels may be derived from the resonance of the chest seems to have little justification. The tone of the chest is a low one—my own has a frequency of about 100 complete vibrations a second—as can readily be determined by singing the scale; the chest resonance occurs only on very low notes. Its low pitch can also be heard by tapping the chest as in auscultation. The chest possibly resonates when very low tones are sung or spoken, but the pitch of ordinary speech is generally quite above it.

I believe we shall not go very far wrong if we assume that the entire mouth cavity may give rise to one resonance tone, the rear portion (pharyngeal) to another and the anterior portion to a third. Such an assumption has been made the basis of my attempt on p. 56 to explain the formation of *ai*.

VI. THE CORD TONE IN VOWELS.

Simple tones have three fundamental properties: pitch, intensity and duration. The so-called "timber" is not a property of simple tones, but the resulting effect of combinations of tones. In the present section it is proposed to discuss the cord tone in various vowels in regard to pitch and intensity. For this purpose only the fundamental tone of the vowel will be considered and no regard will be paid to the particular form of the curve resulting from the overtones of the cord tone and the superposition of the resonance tones. We will also assume that the vibration of the cords involves the usual supposition that the force of attraction to the position of equilibrium varies as the distance from that position. In such a case we can represent the fundamental tone by the equation

$$y = F(t) \sin \frac{2\pi t}{f(t)}$$

[1] PIPPING, *Zur Phonetik d. finnischen Sprache*, Mém. de la Société finno-ougrienne, XIV, Helsingfors 1899.

where $f(t)$ is the expression for the period of the vibration and $F(t)$ that for the amplitude. In this general expression the period and the amplitude may be constant or may vary with the time.

The pitch function.

A vowel during whose course the pitch remains constant can be said to be of "sustained" pitch. If T is the period of vibration of the cords, we have in the ideal case

$$y = F(t) \sin \frac{2\pi t}{T}.$$

Vowels of sustained or constant pitch are not very common in the cases I have studied. Most vowels seem to rise or fall, yet some of them are approximately constant. The vowel *i* as found in *see*, *needle*, *ai*, etc., is approximately a sustained vowel although it generally falls slightly. The following measurements of *i* in *see* are typical: 2.3, 2.3, 2.4, 2.4, 2.8^σ ... to the 22d vibration, 2.4^σ to the 42d vibration, 2.1^σ to the end at the 64th vibration.

The rather unusual case of two vowels of sustained pitch forming a diphthong is found in the word *my* of the phrase *With my bow and arrow.* The *a* has a constant period of 5.6^σ and the *i* that of 3.6^σ. The *a* has also a constant amplitude of 0.4^{mm}; the *i*, beginning with 0.5^{mm}, falls to 0 as usual in *ai* at the end of a word (see Section II.).

The diphthong *ai* is of nearly constant pitch throughout most of its length in the two cases of *thy* (Figs. 62, 67).

Nearly all vowels in the earlier parts of words in the record studied (p. 14), whether preceded by a consonant or not, are characterized by a rising pitch. In such a case the period is not a constant T but a function of the elapsed time, $f(t)$. A typical example of this kind of vowel is found in the *a* of *ai* (Section II.). A determination of the particular form of $f(t)$ for various vowels is a highly important matter, as different vowels and different manners of speaking are possibly characterized by different forms of this rise in pitch. Some of the cases of *a* suggest the form $f(t) = k\epsilon^{mt}$, a formula which expresses many of the phenomena found in nature.

When the rise in pitch (decrease in period) is proportional to the elapsed time, we have

$$y = F(t) \sin \frac{2\pi t}{T_0 - mt}$$

where T_0 is the period of the first vibration and m the factor of proportionality. Such a vowel is found in the *a* of the 4th example of *I* above

(p. 28 and Fig. 29). During an interval of 180^σ its period is shortened by 5.5^σ, or at the rate of $0.03t$. Its cord equation on the suppositions made above would be (in seconds)

$$F(t) \sin \frac{2\pi t}{9 - 0.03t} .$$

In the latter portions of words the vowels in the records I have examined are generally nearly constant in pitch, with often a slight fall as the intensity decreases. Typical examples are found in the cases of *i* in *ai* (Section II.). This slight fall in pitch need not necessarily indicate a relaxation in the tension of the vocal cords; as the force of the expired current of air decreases, the frictional forces involved in the cord vibration may gradually lengthen the period. Yet the amount of fall is generally too great to be due to anything but a relaxation of the cords.

The amplitude function.

The intensity of a sound wave is to be defined as the amount of work performed by the passage of the wave through a unit surface in a unit time. It is directly proportional to the square of the amplitude and inversely proportional to the period. Complete calculations of the intensity of vowels under various circumstances may eventually be made; in the present investigation, however, the amplitude has been taken as the most convenient index of intensity.

In the records studied I have rarely found a vowel with a constant amplitude. Vowels at the beginnings of words show invariably a rise in amplitude. This rise may continue until the vowel ends in some other sound. Such is the case in *a* of *ai* (Section II.), and in *a* of *and* in *thread and needle*. Most vowels, however, rise to a maximum and then fall; as is typically illustrated in *a* (p. 67). Such vowels might possibly be called circumflex vowels. Even in the middle of the word the vowel has a tendency to the circumflex form, as is well shown in most cases of the *i* of *ai*. The rise and fall may be quite elaborate as in the case of the doubly circumflex vowel *o* of *bow*; this long *o*, however, might with propriety be considered a molecular union of two *o*'s in succession.

In a vowel of constant amplitude represented by the sinusoidal vibration we would have $F(t) = a$ and

$$y = a \sin \frac{2\pi t}{f(t)} .$$

In a rising vowel $F(t)$ might take some such form as mt, whence we would have $y = mt \sin \frac{2\pi t}{f(t)}$.

In a circumflex vowel we may assume the amplitude to be of sinusoid form whereby

$$F(t) = E \sin \frac{2\pi t}{s}$$

and

$$y = E \sin \frac{2\pi t}{s} \sin \frac{2\pi t}{f(t)}$$

where E would be the maximum amplitude and s the length of the vowel. When the pitch is constant the curve will have the form

$$y = E \sin \frac{2\pi t}{s} \sin \frac{2\pi t}{T} .$$

I have found one vowel, *a* in *said* in the line *I, said the sparrow,* that can be with close approximation considered as a circumflex vowel of constant pitch. Its equation is (in seconds and millimeters)

$$y = 0.5 \sin \frac{2\pi t}{0.108} \sin \frac{2\pi t}{0.0053} .$$

It does not fill a complete period of circumflexion as it is suddenly cut short by the *s* of *sparrow.*

Among the hundred or so English vowels that I have inspected, I have been unable to find one that can with any close approximation be considered as steady in intensity and constant in pitch. Thus a vowel of the form $y = a \sin \frac{2\pi t}{T}$ must be a rare one. Some vowels during part of their course are of this form, but a change of some kind seems characteristic at some moment. Even such approximations have been found only in the interior of words, that is, with boundaries of consonants or of vowels with the vocal organs already in action. It seems to be the rule in English that a vowel following a pause shall be a rising or crescendo one, and one preceding a pause shall be a falling or diminuendo one.

Sequence of cord tones.

There seems to be for a particular voice on a particular occasion certain tones around which the cord tones group themselves. BOEKE found that in ordinary speech his cord tone ranged from 181 to 256 complete vibrations.[1]

In the first stanza of *Cock Robin* the general tone seems to be one with a period of 5.3^σ (about 190 vibrations).

[1] BOEKE, *Mikroskopische Phonogrammstudien*, Arch. f. d. ges. Physiol. (Pflüger), 1891 L 297.

In addition to this a tone with a period of 7.0^{σ} (about 143 vibrations, making a musical interval of a fourth below the general tone) has a tendency to appear for the sonants of lower pitch and another tone with a period of 1.8^{σ} (about 560 vibrations, making a musical interval of a duodecime above the general tone) for the sonants of higher pitch.

The periods of the various sonants, as far as I have been able to determine them in this stanza, are given in thousandths of a second by the figures below them in the following quotation :

Wh o k i lled C o ck R o b i n
3.3 1.8 4.2 1.8 5.3 5.6 8.4

I, s ai d th e sp a rr ow,
18 to 4 5.3 5.3 5.3 2.8 5.2

W i th m y b ow a nd a rr ow
5.3 2.1 5.3 5.6–3.6 7.0 5.3 4.2 2.5 7.0

I k i lled C o ck R o b i n.
12 to 4 5.6 7.0 to 5.3 3.9 3.9 4.2 5.6 8.8

It may be suggested that the melodiousness of speech must depend to a great degree on the musical sequence of the cord and resonance tones.

VII. Verse-analysis of the 1st stanza of Cock Robin.

As stated on p. 1 these researches were begun in order to settle the controversy in regard to the quantitative character of English verse. A nursery rhyme was selected as being verse in the judgment of all classes of people for many ages. When compared with some of what many of us now consider to be the best verse, it shows various defects, but these defects are typical of the usual deviations from our present standards and are, moreover, not defects according to other standards. It is also a fact that our notions of verse are largely derived from the rhymes heard in childhood.

An analysis of the sounds of the first stanza is given in the four tables on the adjacent pages.

The first column gives the sounds in the phonetic transcription used by Vietor.[1] The second column gives the duration of each sound as determined by measurements of the curves in the records as described on p. 13. The third column gives the period of the cord tone, and the fourth gives the amplitude of the vibration in the tracing (p. 20), not the amplitude of the vibration on the gramophone plate or of the move-

[1] Vietor, Elemente der Phonetik, 3. Aufl., Leipzig 1894.

Line 1: *Who killed Cock Robin?*

Sound.	Duration in thousandths of a second.	Pitch (period in thousandths of a second).	Intensity (maximum amplitude in mm.).	Syllable effect.	REMARKS.
h	>10				Very short sound, not distinguishable in the record, not over 10σ in length. Compare with *h* on p. 60.
ū	189	3.3	0.4	strong	Forcible vowel, large amplitude in earlier portion, rises somewhat in pitch, average period 3.3. Compare with *ū* on p. 63.
k	119				Appears in the record as a straight line.
i	154	1.8	0.6	strong	Long vowel, large amplitude throughout, double circumflex in amplitude (p. 93). The high pitch of this *i* is in contrast with that of *killed* in the 4th line (below)
l	74	1.8	0.1		Compare p. 65.
d	0				No sound of *d* can be heard in this record; the record plate speaks "Who kill Cock Robin?"
k	53				Appears in the record as a straight line.
à	126	4.2	0.5	weak	Rises somewhat in pitch to 4.2 in the main portion, weak on account of lowness in pitch.
*	70				The vibrations of the *à* are suddenly cut short by a few vibrations of a different form that rapidly decrease in amplitude. In listening to the record plate the ear hears no glide between *à* and *k*; the word seems to be simply and distinctly *kàk* and not *kàak*. This glide seems to be, to the ear, an essential part of the *k*. The cords are still vibrating while the mouth is changing from the *à* position to the *k* position.
k	31				Straight line measured from * to *r*; there is no pause between *k* and *r*.
r	74	1.8	0.3		Very distinctly and heavily rolled *r*; pseudo-beats are apparent. Compare p. 69.
à	140	5.3	0.5	strong	Of very low but constant pitch; steady rise in intensity till the vowel is cut short by *b*; forcible on account of length and amplitude.
b	49				Straight line from *à* to *i*. Compare p. 67.
i	56	5.6	0.3	weak	Short but distinctly heard; weak on account of shortness, lowness and faintness.
n	74	8.4	0.2		Falls in pitch and amplitude.
ꜧ	770				

Line 2: *I, said the sparrow.*

Sound.	Duration in thousandths of a second.	Pitch (period in thousandths of a second).	Intensity (maximum amplitude in mm.).	Syllable effect.	Remarks.
ai	452	18 to 4	0.7	strong	Full analysis on p. 16; strong by length, pitch
i	210				of *i* and amplitude.
s	?				Very brief sound, no trace in record.
e	105	5.3	0.5	weak	Rather long and loud, but low in pitch.
d	81	5.3	0.1		Pitch falls from 5.3.
dh	32	?	>0.1		Very weak vibrations.
a	84	5.3	0.2	weak	
sp	273				Impossible to distinguish between the two sounds in the tracing; the *s* is heard as a brief sound.
*	18	1.9	0.4		Distinct sound different from the following *æ*.
æ	170	5.3	0.5	strong	Constant very low pitch but steadily increasing amplitude; falls suddenly in intensity during 5σ to *r*; no sound of *a* as stated in VIETOR, p. 115; strong on account of length and amplitude.
r	11	2.8	0.2		Clearly marked vibrations; the rolling of the *r* can be distinctly heard. Compare p. 69.
ô	294	5.2	0.6	strong	Very long vowel of constant pitch, but of rising and then falling intensity (p. 93); strong by length and amplitude; followed without pause by *ü* of next line

ment of the cords. The fifth column gives what I consider to be the character of each syllable, whether strong or weak; the judgment is based on the sound of the gramophone record, aided by a study of the tables.

The elements in speech whose rhythmical arrangement is the essential of verse as contrasted with prose are: 1, quality; 2, duration or length; 3, pitch; and 4, intensity. The element of quality consists in the nature of the sound as a complex of tones and noises producing a definite effect as a speech-sound. Length, pitch and intensity are properties of the speech-sound that can be varied without destroying its specific nature, that is, without changing the quality. These four elements can be varied independently.

It seems to be sufficiently well settled that, in addition to variations of quality, that is, of the speech-sounds, the essential change in Greek verse was one of pitch. I have observed a similar characteristic in Japanese

Line 3: *With my bow and arrow.*

Sound.	Duration in thousandths of a second.	Pitch (period in thousandths of a second).	Intensity (maximum amplitude in mm.).	Syllable effect.	REMARKS.
ū	108	5.3	0.2		Amplitude rises from o.
i	60	2.1	0.4	strong	Circumflex sustained vowel; compare p. 94; strong by pitch and amplitude.
dh	56	?	0.1		
m	74	5.3	0.1		
ā	179	5.6	0.4	strong	Both parts of this diphthong are nearly constant in pitch and amplitude; compare p. 92; strong by length and amplitude.
i	112	3.6	0.5		
𝄾 *b*	140				*My* is followed by a brief rest in order to bring out the *b* distinctly. The *b* makes no curves in the record.
ō	490	7.0	0.4	strong	Extremely long vowel of very low pitch with two maxima of intensity; it might be considered as a close succession of two *o*'s; compare p. 93; strong by length and amplitude.
𝄾	11				
æ	382	7.7–5.3	0.2	weak	The *æ* begins at a very low pitch 7.7 and rises steadily to 5.3, which is maintained throughout the *n*. The form of the curve for *æ* differs from that for *n*, yet the change is so gradual that it is impossible to assign any dividing line.
n		5.3	0.1		
d	18				Straight line in the record.
*	102	5.3	0.4		This extra vowel arises from the attempt at extra distinctness in speaking.
æ	189	4.2	0.3	strong	Strong by length and pitch.
r	39	2.5 (?)	0.1		Rolled *r*, brief.
o	331	7.0	0.6	strong	A single vowel of circumflex intensity; compare p. 93; strong by length and amplitude.
𝄾	420				

verse. Probably no better way of getting an idea of the nature of Greek verse could be found than that of listening to typical Japanese verse. I have also found another form of pitch-verse in a kind of poetical dictionary used by the Turks for learning Persian.

Latin verse was essentially a time-verse, the chief distinction among the syllables being that of length in addition to the change in speech-sounds.

English verse is usually considered to be an intensity-verse, or a verse of loud and soft syllables. The four tables show quite evidently that English verse is also a pitch-verse and a time-verse.

It may be said that in all probability changes of length and intensity

Line 4 : *I killed Cock Robin.*

Sounds.	Duration in thousandths of a second.	Pitch (period in thousandths of a second).	Intensity (maximum amplitude in mm.).	Syllable effect.	Remarks.
ai	334	12–4	0.6	strong	Full analysis on p. 22; strong by length, pitch of *i* and amplitude.
k	125				Straight line in the record.
i } *l*	324	5.6	0.2	weak	It is impossible to assign any definite point as the limit between these two sounds; weak, low *i* in contrast to the *i* in the first line above.
d	33				This *d* is distinctly heard; compare *d* in first line above.
*	81	4.9	0.2		Additional vowel due to the extra distinctness in speaking the *d*; it arises from the explosive opening of the mouth; the pronunciation of the word *killed* is different from that in the first line chiefly in the great difference in pitch and in the greater distinctness of the *d*.
k	133				Straight line in the record
â	147	7.0–5.3	0.3	weak	Pitch rises from beginning to end.
*	76				See the same word in the first line above.
k	46				Straight line in the record.
r	60	3.9	0.6	strong	The *r* is more vowel-like than the corresponding *r* in the first line; the strong roll is not heard; the curve of *ro* very much resembles in period and amplitude the curve of an *ai* in *thy* (Fig. 61) turned backward; the period of the cord tone is practically constant; the resonance tone of the mouth undergoes a continuous change; any assignment of a limit between the two sounds must be somewhat arbitrary; the sound *ro* is strong by length, pitch and amplitude.
â	103	3.9	0.5		
b	53	4.2	0.1		The *b* cuts off suddenly the sound of *o*.
i	82	5.6	0.4	weak	The *i* is heard, but not so distinctly as in the first line above.
n	74	8.8	0.1		Weak, low, diminuendo.
𝄾	955				

went along with the changes of pitch in Greek verse but that they were of minor importance. Perhaps, also, changes of pitch and intensity likewise accompanied the long and short syllables in Latin verse. But I do not think that for English verse we can fully accept the analogous statement that, although the changes in pitch and length may be present,

they are quite subordinate to the changes in intensity. It would, I believe, be more nearly correct to say that English verse is composed of strong and weak, or emphatic and unemphatic syllables and that strength can be produced by length, pitch or intensity.

The usual scansion of this stanza in strong and weak syllables would give

— — ◡ — ◡
— ◡ ◡ — —
— — — ◡ — —
— ◡ ◡ — ◡

The three elements: length, pitch and intensity, are all used to produce strength. Thus the forcible vowel *ū* in Line 1 is long and moderately high and loud.

The strength of a syllable may be kept the same by increasing one of the factors as another one decreases. The vowel *o* of *Robin* in Line 1 is strong on account of its length and intensity, although its pitch is low. A syllable necessarily short may be made as strong as a longer one by making it louder or higher; or a syllable necessarily of small intensity may be strengthened by lengthening it or raising its pitch. Thus, the short *i* of *With* in Line 3 is strong on account of its high pitch and large amplitude; and the weak *æ* of *arrow* in Line 3 is strong on account of its high pitch and its length. This might be called the *principle of substitution.*

An increase in the loudness, length or pitch of a syllable renders it stronger—other things being equal. Using the symbol f to indicate dependence we may put $m = f(x, y, z)$, where m is the measure of strength and x, y and z are the measures of intensity, length and pitch respectively. This might be called the fundamental *principle of strength.*

The study of this and other specimens of verse has made it quite clear that the usual concept of the nature of a poetical foot is erroneous in at least one respect. *Lines* in verse are generally distinct units, separated by pauses and having definite limits. A single line, however, is not made up of smaller units that can be marked off from each other. It would be quite erroneous to divide the first stanza of Cock Robin into feet as follow.

Who killed|Cock Rob|in?
I, said the|sparrow.
With my bow|and ar|row
I killed|Cock Rob|in.

No such divisions occur in the actually spoken sounds and no dividing points can be assigned in the tracing.

The correct concept of the English poetical line seems to be that of a certain quantity of speech-sound distributed so as to produce an effect equivalent to that of a certain number of points of emphasis at definite intervals. The proper scansion of the above stanza would be:

Who killed Cock Robin?
I, said the sparrow,
With my bow and arrow
I killed Cock Robin.

The location of a point of emphasis is determined by the strength of the neighboring sounds. It is like the centroid of a system of forces or the center of gravity of a body in being the point at which we can consider all the forces to be concentrated and yet have the same effect. The point of emphasis may lie even in some weak sound or in a mute consonant if the distribution of the neighboring sounds produces an effect equivalent to a strong sound occurring at that point. Thus the first point of emphasis in the third line lies somewhere in the group of sounds *mybow*, probably between *y* and *o*.

With this view of the nature of English verse all the stanzas of Cock Robin can be readily and naturally scanned as composed of two-beat or two-point lines.

It is not denied that much English verse shows the influence of quantitative classical models, but such an influence is evidently not present in Cock Robin.

Thanks are due to Prof. HANNS OERTEL who has very kindly read most of the proof of this article; he has enriched it by various suggestions particularly in regard to the *h* discussed on p. 60.

OBSERVATIONS ON RHYTHMIC ACTION[1]

BY

W. SCRIPTURE.

Two entirely different forms of regularly repeated action are to be distinguished. In one form the subject is left free to repeat the movement at any interval he may choose. This includes such activities as walking, running, rowing, beating time, and so on. A typical experiment is performed by taking the lever of a MAREY tambour between thumb and index finger and moving the arm repeatedly up and down; the recording tambour writes on the drum the curve of movement. Another experiment consists in having the subject tap on a telegraph key or on a noiseless key and recording the time on the drum by sparks or markers. Other experiments may be made with an orchestra leader's baton having a contact at the extreme end, with a heel contact on a shoe, with dumb-bells in an electric circuit, and so on. For this form of action I have been able to devise no better name than "free rhythmic action."

In contrast with this there is what may be called "regulated rhythmic action." This is found in such activities as marching in time to drum-beats, dancing to music, playing in time to a metronome, and so on. A typical experiment is that of tapping on a key in time to a sounder-click, the movement of the finger being registered on a drum.

Regulated rhythmic action differs from free rhythmic action mainly in a judgment on the part of the subject concerning the coincidence of his movements with the sound heard (or light seen, etc.). This statement, if true, at once brushes aside all physiological theories of regulated rhythmic action. One of these theories is based on the assumption (EWALD) that the labyrinth of the ear contains the tonus-organ for the muscles of the body. It asserts that vibrations arriving in the internal ear affect the whole contents, including the organ for the perception of sound and the tonus-organ. Thus, sudden sounds like drum-beats or emphasized notes would stimulate the tonus-organ in unison, whereby corresponding impulses would be sent to the muscles. This theory has very much in its favor. It is undoubtedly true that such impulses are sent to the muscles. Thus at every loud stroke of a pencil on the desk I can feel a resulting

[1] Reprinted from Science, 1899 X 807.

contraction in the ear which I am inclined to attribute to the *M. tensor tympani.* Likewise a series of drum-beats or the emphasized tones in martial or dance music seem to produce twitching in the legs. FÉRÉ has observed that, in the case of a hysterical person exerting the maximum pressure on a dynamometer, the strokes of a gong are regularly followed by suddenly increased exertions. Nevertheless, these twitchings are not the origin of the movements in regulated rhythmic action. For many years I have observed that most persons regularly beat time just before the signal occurs; that is, the act is executed before the sound is produced. Records of such persons have been published,[1] but their application to the invalidation of the tonus-theory was first suggested by Mr. Ishiro MIYAKE. This does not exclude the use of muscle sensations, derived from tonus-twitches, in correcting movements in regulated rhythmic action, although they presumably play a small or negligible part as compared with sounds.

Another argument in favor of the subjective nature of regulated rhythmic action is found in the beginning of each experiment on a rhythm of of a new period; the subject is quite at loss for a few beats and can tap only spasmodically until he obtains a subjective judgment of the period. If the tonus-theory were correct, he should tap just as regularly at the start as afterward.

The conclusion seems justified that regulated rhythmic action is a modified free rhythmic action, whereby the subject repeats an act at what he considers regular intervals, and constantly changes these intervals to coincide with objective sounds which he accepts as objectively regular.

In free rhythmic action there is one interval which on a given occasion is easiest of execution by the subject. This interval is continually changing with practice, fatigue, time of day, general health, external conditions of resistance, and so on.

"It has long been known that in such rhythmic movements as walking, running etc., a certain frequency in the repetition of the movement is most favorable to the accomplishment of the most work. Thus, to go the greatest distance in steady traveling day by day the horse or the bicyclist must move his limbs with a certain frequency; not too fast, otherwise fatigue cuts short the journey, and not too slow, otherwise the journey is made unnecessarily short. This frequency is a particular one for each individual and for each condition in which he is found. Any deviation from this particular frequency diminishes the final result."

It is also a well-known fact that one rate of work in nearly every line is peculiar to each person for each occasion, and that each person has

[1] SCRIPTURE, New Psychology, 182, London 1897.

his peculiar range within which he varies. Too short or too long a period between movements is more tiring than the natural one in walking, running, rowing, bicycling, and so on.

It is highly desirable to get some definite measurement of the difficulty of a free rhythmical action. This cannot well be done by any of the methods applicable to the force or quickness of the act, but it may be accomplished in the following manner.

As a measure of the irregularity in a voluntary act we may use the probable error. When a series of measurable acts are performed they will differ from one another, if the unit of measurement is fine enough. Thus, let $x_1, x_2, \cdots, x_n$ be successive intervals of time marked off by a subject beating time, or walking, or running, at the rate he instinctively takes. The average of the measurements,

$$a = \frac{x_1 + x_2 + \cdots + x_n}{n}$$

can be considered to give the period of natural rhythm under the circumstances. The amount of irregularity in the measurements is to be computed according to the well-known formula:

$$p = \sqrt{\frac{v_1^2 + v_1^2 + \cdots + v_n}{n - 1}}$$

where $v_1 = x_1 - a$, $v_2 = x_2 - a$, $\cdots$, $v_n = x_n - a$. The quantity p is known as the "probable error," or the "probable deviation." The quantity

$$r = \frac{p}{a}$$

the "relative probable error," expresses the probable error as a fraction of the average.

If all errors in the apparatus and the external surroundings have been made negligible, this "probable error" is a personal quantity, a characteristic of the irregularity of the subject in action. If, as may be readily done, the fluctuations in the action of the limbs of the subject be reduced to a negligible amount, this probable error becomes a central, or subjective, or psychological, quantity. Strange as it may appear, psychologists have never understood the nature and the possibilities of the probable error (or of the related quantities "average deviation," "mean error," etc.). In psychological measurements it is—when external sources of fluctuation are rendered negligible—an expression for the irregularity of the subject's mental processes. Nervous or excitable

people invariably have large relative probable errors; phlegmatic people have small ones.

Thus a person with a probable error of 25% in simple reaction time will invariably have a large error in tapping on a telegraph key, in squeezing a dynamometer, and so on. I have repeatedly verified this in groups of students passing through a series of exercises in psychological measurements. I do not believe it going too far to use the probable error as a *measure* of a person's irregularity. This is equivalent to asserting that a person with a probable error twice as large as another's is twice as irregular, or that if a person's probable error in beating time at one interval is r_1 and at another interval r_2, his irregularity is r_1 times as great in the second case as in the first. This concept is analogous to that of precision in measurements. We might use the reciprocal of the probable error as a measure of regularity. The positive concept, however, is in most minds the deviation, variation or irregularity, and not the lack of deviation, the non-variability, or the regularity. In the case of the word "irregularity" the negative word is applied to a concept that is naturally positive in the average mind.

The irregularity in an act is a good expression of its difficulty. Thus, if a person beating time at the interval T has an irregularity measured by the probable error P and at the interval t a probable error p it seems justifiable to say that the interval t is $\frac{p}{P}$ times as difficult as T. If T is the natural interval selected by the subject, then the artificial interval t would be more difficult than T, and we should measure the difficulty by comparing probable errors.

It is now possible to state with some definiteness the law of difficulty for free rhythmic action. Let T be the natural period and let its probable error—that is, its difficulty—be P. It has already been observed (Science, 1896, N. S. IV 535), that any other larger or smaller period (slower or faster beating) will be more difficult than the natural one and will have a larger probable error. Thus any interval t will have a probable error p which is greater than P, regardless of whether t is larger or smaller than T.

Three years ago (Science, as above) I promised a complete expression for this law. Continued observations during this time enable me to give an idea of its general form. The results observed can be fairly well expressed by the law

$$p = P\left(1 + c\,\frac{[t - T]^2}{t}\right)$$

in which T is the natural period, P the probable error for T, t any arbitrary period, p the probable error for t and c a personal constant.

This may be called the law of difficulty in free rhythmic action. A curve expressing the equation for $T = 1.0^s$, $P = 0.02^s$ and $c = 1$ is given in the figure.

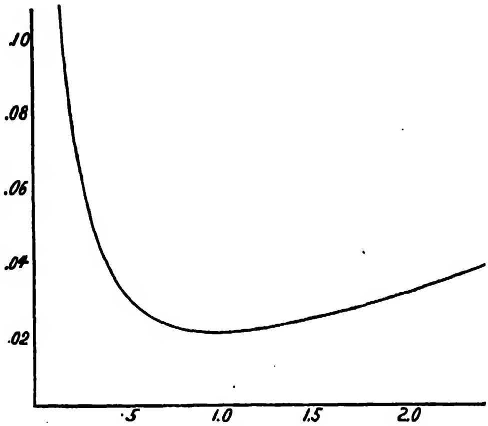


Fig. 75.

It will be noticed that periods differing but little from the natural one are not much more difficult and that the difficulty increases more rapidly for smaller than for larger periods.

In plotting this curve I have assumed unity as the value for all personal constants. The personal constants will undoubtedly vary for different persons, for different occasions and for different forms of action; an investigation is now in progress with the object of determining some of them.

In case it is desired to know what periods are of a difficulty 2, 3, ⋯, n times that of T, a table of values for p may be drawn up in the usual way and that value for t sought (with interpolation) which gives for p a value 2, 3, ⋯, n times as great. Thus, in a table for the above example it is found that the periods 0.38^s and 2.6^s are twice as difficult.

This law can be stated in another form which is of special interest to the psychologist. To the person beating-time a period of 0 is just as far removed from his natural period as one of ∞; both are infinitely impossible. The objective scale does not express this fact; objectively a period of 0 is as different from a period of 1^s as a period of 2^s would be. Similar considerations hold good for the lesser periods; the scale by which the mind estimates periods is different from their objective scale. This difference may be expressed by asserting that the following relations exist between the two:

$$x \doteq c\,\frac{(t-T)^2}{t}$$

where x is the measure on the mental scale, T the natural period, t any other period, and c a personal constant. By this formula the various periods may be laid off according to their mental differences from the natural period. Every difference from the natural period is mentally a positive matter. With the mental scale the law of difficulty becomes

$$p = P\,(1 + cx)$$

where p and P are the probable errors for t and T respectively, x is the measure on the mental scale and c is a personal constant. This is the equation of a straight line. The law states that the difficulty of any arbitrary period is directly proportional to its mental difference from the natural period. This is the statement which I tried to make in the note published in Science, 1896, N. S. IV 535.

This law of difficulty as depending on the period is, of course, only one of the laws of free rhythmic action. It is quite desirable that other laws of difficulty and of frequency should be determined. For example, observations on ergograph experiments tend to show that the irregularity and the natural period both change with the weight moved; they also change with the extent of the movement.

Such a series of well established laws might be useful in regulating various activities to the best advantage. It is already recognized that it is most profitable to allow soldiers on the march to step in their natural periods; it is also known that on the contrary sudden and tense exertion is favored by changing the free rhythmic action into regulated action by marching in step and to music. More definite knowledge might perhaps be gained concerning the most profitable adjustments of the rhythm and extent of movement in bicycle-riding to the person's natural period; at present only average relations are followed in the adjustment of crank-length, gear and weight to bicycle-riders, individual and sex differences not being fully compensated. Other examples will suggest themselves.

Not only does every simple activity have its own natural rhythms; combinations of activities have rhythms that are derived from the simpler ones. In fact, it may be said that the individual, as a totality, is subjected to a series of large rhythms for his general activity (e. g., yearly, monthly, weekly, daily, and so on), and also to a series of smaller rhythms for his special activities. The natural periods do not always correspond with the enforced periods. The daily rhythm is unquestionably too slow for some persons and too rapid for others; the unavoid-

able enforcement of the 24-hour period works a loss to all who would naturally vary from it, and diminishes the total amount of work that could be produced by them. For large numbers of brain-workers the 24-hour period is too long; for many of them the natural period is probably about 18 hours. Although about one-quarter of the day is not efficiently used, there is little relief in splitting up the day into parts, because (1) the 12-hour period would be naturally even less advantageous than the 24-hour one, and (2) the new rhythm cannot be made to fit the environment.

The progress of civilization and the changes in life are undoubtedly tending to shorten the natural period from 24 hours by encouraging a greater discharge of energy at shorter intervals. Since the 24-hour rhythm is a fixed one, there must be a constant effort at adjustment in this respect by those individuals most susceptible to the new influences. the survival of the fittest will, of course, tend to keep the natural rhythm not far from the 24-hour period.